PHILOSOPHICAL TRANSACTIONS.

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies and *Labours*

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the World.

VOL. XXXIV. For the Year 1726, and *Jan. Febr. March, April, May* and *June* 1727.

LONDON,

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Jos: Banks

TO

Martin Folkes, *Esq*;

Vice-President of the Royal Society.

Honoured Sir,

I Shall not, I presume, need any other Apology for prefixing your Name to this *Thirty Fourth Volume* of *Philosophical Transactions*, when I declare, that the Motive of my doing so was the same, which induc'd the greatest Man that ever liv'd, to single you out to fill his Chair, and to preside in the Assemblies of the *Royal Society*, when the frequent Returns of his Indisposition would no longer permit him to attend them with his usual Assiduity.

This Motive, Sir, we all know, was your uncommon Love to, and your singular Attainments in those noble and manly Sciences, to which the Glory of Sir *Isaac Newton*, and the Reputation of the *Royal Society* is solely and entirely owing.

That Great Man was sensible, that something more than knowing the Name, the Shape and obvious Qualities of an Insect, a Pebble, a Plant, or a Shell, was requisite to form a Philosopher, even of the lowest rank, much more to qualifie one to sit at the Head of so great and learned a Body. We all of us remember that Saying so frequently in his Mouth, That *Natural History might indeed furnish Mate-*

rials for Natural Philoſophy; but, however, Natural Hiſtory was not Natural Philoſophy; and it was eaſy for all his Friends to ſee, with what intent he ſo often us'd this remarkable Expreſſion. We knew his Love to the *Royal Society*, and his Fears for it. It was not that he deſpis'd ſo uſeful a Branch of Learning as *Natural Hiſtory*; he was too wiſe to do ſo: But ſtill he judg'd that this humble Handmaid to Philoſophy, tho' ſhe might be well employ'd in amaſſing Implements and Materials for the Service of her Miſtreſs, yet muſt very much forget her ſelf, and the Meanneſs of her Station, if ever ſhe ſhould preſume to claim the Throne, and arrogate to her ſelf the Title of the *Queen of Sciences*.

Theſe, Sir, were the Sentiments of that Illuſtrious Philoſopher, and his Authority alone would weigh a great deal with you, if the Solidity of your own Judgment did not convince you of the Truth of them. But I ſhall not offer here to draw your Character: It is ſufficient to ſay, that Mr. *Folkes* was Sir *Iſaac Newton*'s Friend. However, while I avoid being troubleſome to your Modeſty, I beg leave to indulge my own Vanity thus far, to endeavour by this publick Application to perpetuate the Memory of that ſincere and conſtant Friendſhip, which has now ſo many Years ſubſiſted between you and me, who am, with the higheſt Eſteem and Reſpect, Sir,

Your moſt Obliged and moſt Faithful Servant,

J. JURIN, R. S. *Secr.*

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PHILOSOPHICAL TRANSACTIONS.

For the Months of *January* and *February*, 1726.

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Jos: Banks

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I. De Salibus Alcalino-fixis, *Auctore* C. Neumàn, *Chym. Prof. Reg.* Berolin. *R. S. S.*

SALIA præcipuum totius chymiæ fulcimentum conſtituere nemini ignotum, eorumque fundamentalem cognitionem illi, qui vere utile quicquam cum iis inſtituire & peragere in chymia cupit, præ reliquis, circa quas verſatur chymia, conſuetis materiis, comparandam eſſe, extra omnem dubitationis aleam poſitum eſt: Quod vero ad hujus modi veram fundamentalem cognitionem, neque materiarum, ope inſtrumentorum geometrico-mechanicorum, in minutiſſimis particulis commenſuratione, neque externa contemplatione cum artificioſiſſimis etiam microſcopiis inſtituta, imo nec reliquorum ſenſuum externorum auxilio pervenire quis poſſit, chymici diligentes & laborioſi nimium jam experti ſunt; ita ut hodie cum ſcientia hacce demonſtrativa, Deo favente, eo perventum ſit, ut ad huius ſubjecta pariter & objecta, experientia infallibili comprobata, cenſenda & dijudicanda, illa, quæ ante multos annos irrepſit, ad alias forſitan ſcientias ſatis idonea, ad noſtram vero, licet docta valde & ſpeculativa apparens, tota inepta & minus ſufficiens philoſophia corpuſcularis, & per eam inſtitutæ mechanico-phyſicæ conſiderationes & ratiocinia, penitus nunc abrogata ſit & deleta. Hujus in locum (juxta effatum Senecæ: Veritas eſt ſimplex) alia, primo licet intuitu ſimplex viſa, attamen veritate & experientia ſuffulta, fallere neſcia, & unicuique comprobata methodus, ad chymica dicta phænomena dijudicanda conſtituta eſt, quæ veram & internam na-

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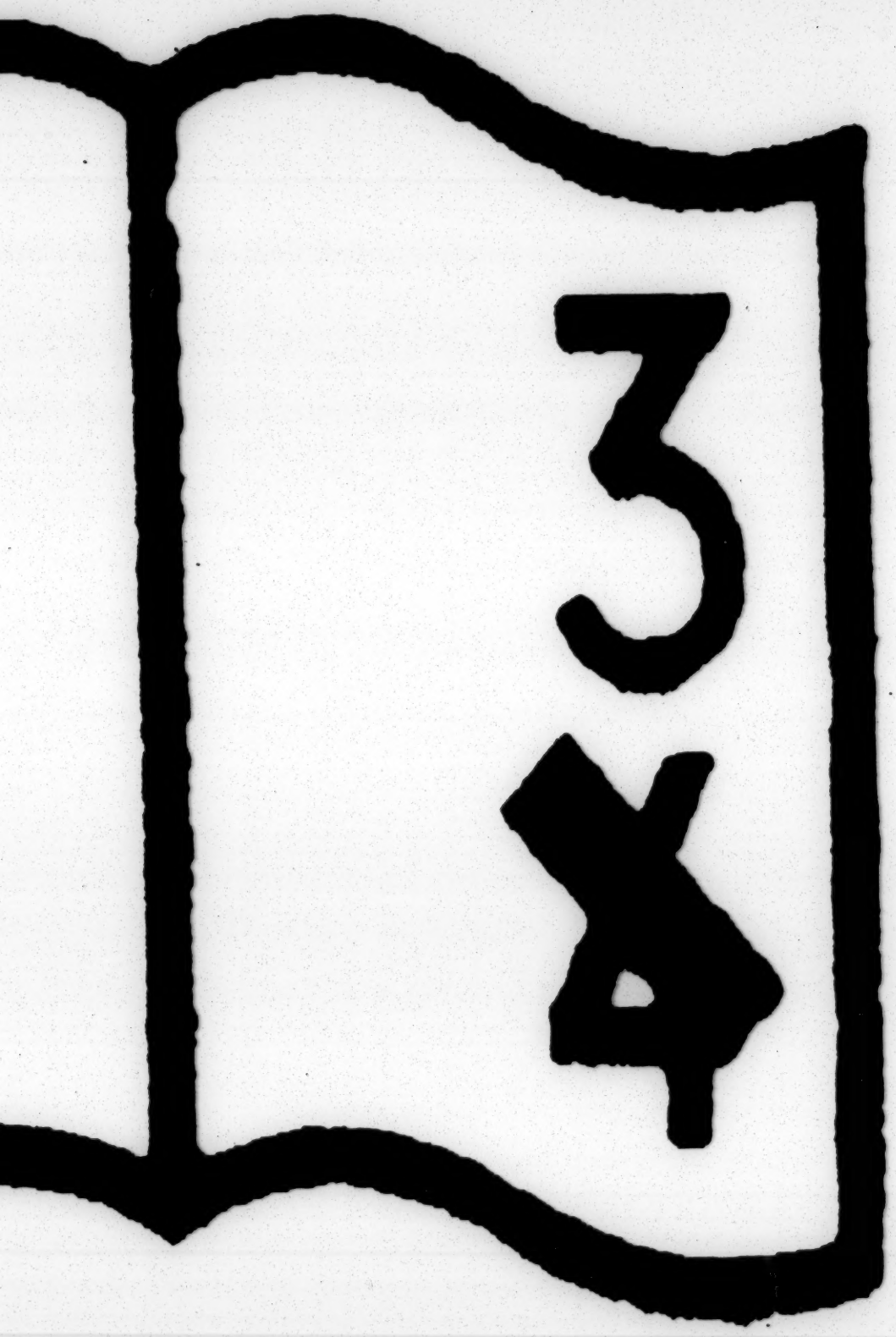

turam partium conſtitutivarum, quæ in omnibus a Deo creatis, & quæ noſtra hæc ſcientia ſibi vendicat, corporibus mixtis reperiuntur, clariſſime, quouſque humani ſcilicet ingenii limites pertingunt, enucleatam & intellectui perviam exhibet. Adeo ut quilibet, ope hujus ab experientia profectæ theoriæ, lætus ſe munitum ſentiat, non modo obvias quaſlibet materias naturales, abſque dubii cujuſvis ſuſpicione clare cognoſcendo & dijudicando, verum etiam eaſdem, chymiæ auxilio, diverſimode reſolvendo vel combinando, aut nova mixta iterum producendo.

Cum vero in meis laboribus (quibus per viginti novem jam annos incubui) nec meliorem, nec magis ſufficientem viam, quam illam modo memoratam, ſine ulla præconcepta opinione viderim atque expertus ſim; itaque hancce hic congeſtam & propria experientia fundatam *de Salibus Alcalino-fixis* deſcriptionem, & quicquid de iis ſcire quam maxime refert, dicta methodo proponam: Nec minus etiam illas cum hiſce concurrentes materias, phænomena & præparata eatenus dijudicabo, quatenus ſapientia divina me hiſce donavit hucuſque, & quatenus reliqua mihi mandata multifaria negotia permittent. In quibus omnibus unicuique ſuam, in materia hacce, cum mea diſcrepantem opinionem lubens relinquam, hiſce conſiderate declaraturus, me ſemper aliorum opinioni calculum adjecturum, modo illa, magis quam mea, experientiæ reſpondens, me penitus convincat.

I. *De horum Salium Etymologia.*

Mixta illa jam conſideranda ut plurimum *Salia Alcalina*, *Salia fixa*, vel *Salia Lixivioſa* appellantur. 1. *Salia Lixivioſa* audiunt, quia per elixationem acquiruntur. 2. *Salia* vero *fixa* dicuntur, quoniam maxi-

ma ignis vi nec volatilia redduntur, neque in ſublime feruntur vel abiguntur. 3. Quicquid per *Alcali* denominationem intelligitur, non adeo ignota etiam res eſt, nempe oppoſitum acidi, vel quod idem eſt, *Antacidum*. Origo autem nominis hujus ab arabico *Alcali* derivatur; *Kali* ſignificat Sal, & *Al* præfigitur, quemadmodum in Gallica lingua articulus definitus *le*, *la*; itaque *Al Kali* conjunctim *le Sel* denotat. Notum inſuper eſt, quod & herba quædam reperiatur, *Kali* dicta, in locis maritimis creſcens, multoque ſale ſcatens, à *Lobelio* etiam *Soda* nuncupata, quæ ſi comburitur & elixatur, ingentem ſalis copiam præbere ſolet, quod olim ſimpliciter *Sal* adjecto Arabico nomine *Alcali* appellatum fuit. Cumque hoc ipſum Sal combuſtioni etiam aliarum herbarum & vegetabilium forte anſam dederit, & ſales inde prolecti ejuſdem fere indolis & naturæ reperti fuerint, credibile admodum eſt, veteres, modo dicti ſalis originale nomen ideo retinuiſſe, & hoc illudve æque *Alcali* appellaſſe, & tantum tractu temporis, ad meliorem diſtinctionem, hoc vel illud cognomen adjectum forſitan fuiſſe, ita præter *purè Salia Alcalia*, corpora etiam *terreſtria Alcalia* innotuere; prioribus, pro noſtro nunc ſcopo nobis inſervientibus, ad diſtinctionem terreſtrium, latinam denominationem *Salis* adjecerunt, & hocce in aqua ſolubile mixtum *Sal Alcali*, vel in genere *Salia Alcalia* appellarunt, ne forte ſola vox *Alcali* æquivoca evadat, vel ad ſignificationem ambiguam, inter *Salinum* vel *terreſtre Alcali*, occaſionem præberet.

In genere autem de adpellatione monendum, non ſatis exprimendi efficacem eſſe denominationem, ſive ſolùm *Salia Alcalina*, ſive *Salia fixa* ſolùm vocentur, quoniam utrumque nomen ſenſum iterum ſuggerere poteſt æquivocum: etenim primo habemus *& Salia Alcalia volatilia*, & ſecundo *Salia fixa*, quæ non *pure*

Alcalina, ſed *Salia* dicta *Neutra* conſtituunt: Quamobrem ſecurius erit, præparata iſta nunc conſideranda ambobus ſimul inſignire nominibus, puta *Salia Alcalina fixa* eadem appellare.

Ceterum nobis ſufficit ſcire, quod voce *Alcali* oppoſitum *Acidi*, per nomen *fixum* vero aliquid, quod conſtans in igne ſit, & per maximam ignis torturam, ita per ſe, non penitus deſtrui vel auferri poſſit, intelligatur, & quod in toto orbe, a multis retro ſeculis, in hodiernum uſque diem uſui ceſſerit, ſalia ejuſmodi, ad diſtinguendum voces in chymia, ita nuncupare & ab aliis ſalibus diſtinguere. Parum denique refert chymico practico, num voces ejuſmodi Arabicæ, vel Chaldaicæ, vel alii omnino linguæ originem debeant, modo verſus alios artis peritos loquendo vel ſcribendo mentem ſuam explanare poterit.

Originarium Alcali hodie quidem adhuc conficitur, ſed non ex unica herba *Kali* vel *Soda* tantum, ſed ex aliis ſimul ad maris littora creſcentibus, nec ſale deſtitutis herbis. Cum vero maximam partem ſalis hujus ſoda vel *Kali* forſitan porrigat, ideo ſub nomine ſodæ, & cum copioſiſſime in Hiſpania alitur & præparatur, ſub cognomine *Sodæ Hiſpanicæ* exportatur, venditur & conſumitur.

Quemadmodum vero *Kali* hocce non parum de ſale marino participat, cum quo, uti dictum, & aliæ herbæ permiſcentur, totaque incineratio & ſubſequens præparatio non adeo circumſpectè, ſed ſolummodo ab hominibus plebejis ejuſmodi locorum accolis peragitur; itaque ultro apparet, quemadmodum & re ipſa conſtat, *Alkali* hocce vel ſodam minime purum chymicum, ſed aliis ſalibus & impuritatibus, ut & abundanti terra conſpurcatum Sal Alkali exhibere: Quare & noſtris hiſce temporibus nec chymiæ, neque medicinæ, ſed ſolummodo mechanicis artificibus & opificibus,

bus, maxime vitriariis, ſaponum confectoribus & tinctoribus inſervit. Si quis vero, hiſce non obſtantibus, illo ad chymicos labores uti vellet, tunc depuratione neceſſaria & ſeparatione earum, ad Sal Alkali purum haud pertinentium, impurarum & peregrinarum ſive admixtarum, ſive caſu introductarum rerum omnino indiget. Hæcce omnia, occaſione originis vocis Alkali, paucis præmittere volui, ad proprium ſcopum nunc reverſurus.

II. *Ex quibus Salia Alcalina fixa pura ſint cognoſcenda, & quibus proprietatibus gaudere debeant.*

Sal Alkali fixum purum primo quatuor proprietates Alkali, cujuſdam in genere poſſideat neceſſe eſt, quibus & ſalia Alcalino-Volatilia ſtipata ſunt.

1. Ut cum acidis quibuſvis efferveſcat.
2. Ut cum acidis hiſce in Sal medium abeat.
3. Ut omnes cum acidis factas ſolutiones præcipitet.
4. Ut cœruleum ſyrupi violarum colorem in viridem tranſmutet.

Sal Alcali fixum contra in ſubſequentibus proprietatibus a volatilibus Alcalicis ſalibus quam maxime diſcrepat, nempe quod

1. Solutionem Mercurii ſublimati non in album, quod ſalis Alcali volatilis indoles eſt, ſed in fuſcum, citrinum colorem præcipitet.
2. Cum cupro adjecto nec veſtigium coloris cœrulei excitet.
3. Cum Spiritu Nitri fumum haud producat.
4. Aquæ admixtum, non frigoris, ſed potius caloris ſenſum illi conciliet.

5. Ab

5. Ab aere admiſſo haud ſiccum relinquatur, ſed humidum evadat, vel denique liqueſcat.
6. In igne non avolet, ſed permaneat, candeſcente ſuaviter igne fere nihil, ingenti vero & fuſorio, parum admodum de pondere ſuo amittat.
7. Non ſolum in igne fixum perſeveret, ſed etiam cito ſe fundi patiatur, & denique
8. In Cryſtallos haud abeat quemadmodum ſalia nonnulla volatilia.

Præterea Sal Alkali fixum purum, etiam ſubſequentes qualitates peculiares poſſidere, & in tentaminibus monſtrare debet.

1. Ut in forma ſoluta, cum alio, circa quod occupati ſumus, puriſſimo ſale Alcali fixo, abſque ulla alteratione, turbatione vel præcipitatione, amicabiliter coëat neceſſe eſt.
2. Ut Sulphur in humida via, æque ac ſicca, omnino ſolvat oportet.
3. Cum Acido vitriolico puro permixtum, & in ſal medium cryſtallinum coactum, in igne nihil ponderis ſui amittere, nec ullo modo fluere debet.
4. Denique purum & genuinum ejuſmodi Sal Alcali fixum, præter reliquas omnes antea recenſitas 15 proprietates, cum pinguibus etiam vel expreſſis oleis ſaponem conſtituere neceſſum habet.

Quod ſi vero in unico ſolum harum ſingularium commemoratarum proprietatum peccet, tunc ſtatim ſignum penitus infallibile eſt Sal Alkali fixum non purum fuiſſe.

III. *Quænam ſubjecta requiruntur ad producendum Sal Alcali fixum.*

Sicuti Chymia univerſa circa res pure Naturales verſatur, & in genere nil aliud peragit, quam reſolvere & combinare, nil quoque producit, ubi nihil exiſtit, ſed ſolummodo in hunc vel iſtum modum, de his jam dum præſentibus ſubjectis, nova omnino mixta, per tranſpoſitionem partium ut plurimum (ſive reſolvendo id fiat vel combinando) artificioſe producit, quæ natura, in ſe ſpectata, defectu inſtrumentorum, ſive nunquam, ſive tantum rariſſime exhibere poteſt; & ſic omnia ad naturæ nutum in Chymia peraguntur, licet difficile admodum ſit de omnibus & ſingulis caſu productis novis, veram & phyſicalem cauſam in inſtanti eruere. Ita de Salibus hiſce Alcalino-fixis in anteceſſum ſcire convenit, ea pariter naturali penitus & comprehenſibili modo fieri.

Quantum mihi innotuit, natura cum integro ſuo penu, nullum nobis ſatis purum ſeparatum, & cum recenſitis proprietatibus dotatum, Sal Alcali fixum porrigit, ſed exiſtit, ſi demonſtrabile aliquod reperitur, (qualecunque in toto orbe Sal Alcali nativum invenire & explicare poſſum) unice & ſolum in illo, cum ſale Acido jam permixto, communi vel marino ſale. Circa hoc vero conſiderari oportet, quod Sal Alcali fixum ejuſmodi,

1. Quia illud nunquam nulloque modo in ſtatu ſuo pure Alcalino, ſed ubique, ſive cum acido ſalis communis, vel etiam, quemadmodum in fontibus nonnullis ſotericis, cum Acido Vitriolico conſpurcatum reperitur; ſi denique illud demonſtrare cupio, (poſito me hoc demonſtraturum)

variis

variis nihilominus artificiis ab admixtis ejuſmodi rebus liberari prius debet; ita ut talia, artificioſis laboribus educta corpora, (certo reſpectu) haud ſimpliciter pro pure nativis, circa quæ ars minime verſatur, declarare poſſim.

2. Quod hocce in Sale communi reperiundum Alcalinum ens, cum denique illud artificiis variis ſeparaverim & demonſtraverim, nihilominus cum antea recenſitis ſedecim proprietatibus perfecte haud congruens purum, ſed a ſale Alcali fixo vegetabili ordinario valde diſcrepans Alcali exiſtat. Ex quibus generaliter denique concludo, purum Sal Alcali fixum naturam minime nobis porrigere, conſequenter omnia Salia Alcalina pura tota ARTIS PRODUCTA eſſe.

Regnum Vegetabilium, vel hiſce adnumeratas res naturales, ſi non in totum, maximam tamen partem, pro ſubjecto & materia agnoſcunt, ex quibus in maxima copia produci & elaborari queunt; Nam licet hinc inde parum quicquam demonſtrativi, in Regno etiam Animali exhiberi quandoque poſſit, attamen, excepto illo, in urina & fæcibus a dicto ſale communi dependente, in tam parva quantitate exiſtit, ut attentionem haud mereatur. Rationem, quare tam parum, vel nihil Alcalino-fixi exibeat, ſuo loco, pro viribus, paucis ſubjungam.

Mirum interea videtur & paradoxum, quando de origine & exiſtentia Salis Alcali dicimus, produci illud ex re quadam, quæ nec granum Salis Alcalino-fixi continuit; quemadmodum ex omnibus iſtiuſmodi rebus, ex quibus multæ libræ imo centenarii Salis Alcali fixi eliciuntur, ante operationem in igne, vel ante incinerationem vel combuſtionem, nec granum hujuſmodi, in tentaminibus ſingulis probati Salis Alcalini

calini ad ſenſum educi poteſt; ſed ſi ſalini aliquid detegitur, ad acidam potius, quam Alcalinam naturam verget.

Majorem multo attentionem ſibi vendicat paradoxum hocce, ſi quis multa, ne dicam plurima, alia præparata Chymica conſueta cum hiſce comparat, in quibus ſingulis apparet, quod illud, quod Chymicus eduxit, antea jam in ſubjecto extiterit, & in illo ſolummodo extenſum, aliiſque rebus permixtum fuerit; conſequenter ab artificibus ſolummodo a permixtis ſeparatum, ſicque collectum, non productum, ſed eductum, nec de novo creatum vel compoſitum, ſed ſolum extractum vel ſegregatum fuiſſe; quemadmodum, exempli loco, in oleis eſſentialibus & expreſſis, in illis ope balnei eductis vegetabilium aquis, ſalibus eſſentialibus, ſpiritibus mineralibus, multiſque aliis rebus videre eſt: Verum illud ipſum de permultis aliis, & quidem proprie chymico-artificialibus rebus, *ex. gr.* de ſpiritu vini, ſpiritibus inflammabilibus, quibuſvis acetis, &c. & eadem quoque ratione de Salibus Alcalinis tam fixis, quam volatilibus, item de oriundis exinde ulterius novis mixtis, ut ſalibus ammoniacalibus, ſaponibus & ſimilibus aliis, dici nequit; ſi quidem de omnibus his rebus, & ſic etiam de Sale Alcali fixo, in ſubjecto ex quo id præparatum fuit (ut antea jam monui) ne micula quidem ejus antea in ipſo reperiri potuit, unde quoque Sal Alcali Fixum non eductum, ſed revera *productum* & *artificiale* exiſtit.

Præcipuum & maxime neceſſarium inſtrumentum ad productionem Salis Alcalino-fixi eſt IGNIS, utpote in quo prærogativa artis præ natura in tantum conſiſtit, dum hunc pro lubitu & placito excitare, minuere & augere, variaque corpora cum ipſo reſolvere & componere valet, quomodo & quando ipſa cupit: & cum hoc, nempe quod Salia Alcalino-fixa mediante

igne producantur, quidam Chymici verbales forte audiverint, hi derepente nudum inſtrumentum pro materia Salis Alcalini, vel pro principio conſtitutivo ipſo venditarunt, & univerſam Salis Alcalino-fixi formationem particulis ignis adſcripſerunt, cum tamen quotidiana experientia plane diverſum, & quod ignis undique tantum ut inſtrumentum ad ejus formationem cooperetur, viſibiliter convincat & demonſtrat, prout & ipſe in ſequentibus de hac re ulterius mentem meam ſum explicaturus.

Subjectum, ex quo Sal Alcali fixum produci debet, proprie duo neceſſaria requiſita vel ingredientia continеat, neceſſe habet, quæ ad conſtitutionem hujus ſalis abſolute requirantur, nimirum ut,

1. In ſe ſit inflammabile, & oleoſis vel bituminoſis partibus, &
2. Sale quoque acido dotatum.

Quod ſi ergo has abſolute & neceſſario requiſitas proprietates in uno ſubjecto conjunctas reperio, tum ſatis quoque ſufficiens eſt, ut Sal Alcali-fixum largiri queat, unde omnia fere vegetabilia, vel eorum partes incorruptæ, aptiſſima exiſtunt; uno vero utrorumque deficiente, parum quoque vel nihil Salis Alkali fixi inde emerget.

Hoc ergo fundamento omnis Salis Alcalino-fixi in mundo productio innititur, & pro diverſitate proportionis horum memoratorum duorum neceſſariorum ingredientium, etiam ex hoc vel illo ſubjecto modo plus, modo minus, modo debilius, modo fortius, vel alio modo diverſum Sal Alcalino-fixum acquiritur.

Sola ſiquidem oleoſi ingredientis copia omne punctum non abſolvit, dum & inprimis ſal acidum ut adſit, neceſſe eſt.

Quod

Quod ſi ergo Sal Alcalino-fixum productурus, de quodam vegetabili, ſive ſit herba, radix, lignum, cortex, vel quicquid ejus ſit, antea tantum per alios labores, per quos ſolummodo contentam in eo olei & ſalis acidi proportionem concludere poſſum, ſufficienter inſtructus ſum, tum in anteceſſum quoque judicare potero, an multum vel parum Salis Alcali fixi daturum ſit: Quod ſi enim parum vel nihil olei poſſidet, parvam quoque Salis Alcali proportionem ſubminiſtrabit, & ſi parum vel nihil ſalis acidi contineat, vel acidum volatilius exiſtat, multo minus adhuc ſuppeditabit; unde poſterior hæc circumſtantia, plerumque in cauſa & culpa eſt, quod non raro ex magno cujuſdam ligni acervo, vel aliis vegetabilium partibus, parum ſalis Alcali erui poſſit; & ſicut indubitatum exiſtit, quod omne ſanum vegetabile partes inflammabiles contineat, ita contra multa ex iis, vel parum ſalis, vel etiam volatile & ſub incineratione plerumque diffugiens, ſal acidum continent.

Dari quoque poſſunt plantæ, ligna, radices & ſimilia, ſalis & olei ditiſſima, quæ vero, dum incinerari debent, propter nimiam aeris intercluſionem, tantummodo vaporant, & in carbones, non vero in cineres exuruntur; qua circumſtantia omne fere ſal acidum, quamvis ejus non parvam quantitatem contineant, (præcipue ſi vegetabile adhuc viride, & inſufficienter exſiccatum ſit) ſenſim ſenſimque exhalat, perparum reſtat, & omne quod remanet, cinerem in pauca, & Sal Alcali-fixum in longe minori adhuc quantitate ſuppeditat.

Res hæc demonſtrari poteſt, *ex. gr.* in Pinu, vel quovis ligno, quo communiter ad urendum utimur: Si ejus aliquot centenarii recipiantur, iique leniter aperto aere comburantur, ſufficientem cinerem, & ex his notabilem quoque ſalis partem nanciſcemur:

Si vero ejuſdem ligni totidem centenarii in parvos acervos conſtruantur, carbonariorum more, vel alio quoque modo, ligno apertus aer adimatur, ejuſque modo tantum ei permittatur, ut vaporare poſſit, breviter carbones conſueti exinde conficiuntur & quidem ea ratione, ne ulli carbones inflammantes, qui in igne, aperto aere, fumantem flammam edunt, reſtent; ejus modi puros carbones, ſi tum aperto aere incineres, eoſque in comparatione quantitatis priorum cinerum ponderes, demum Sal Alcali exinde modo conſueto, elixiviando ſepares, & itidem ponderes, hæc poſterior methodus immenſum quanto minus cinerum & ſalis in reſpectu prioris, quod ab initio ſtatim aere aperto incineratum fuit, ſuppeditabit. Nulla alia ex ratione, hæc ponderis diverſitas ex ejuſdem quantitatis ligno oritur, quam quod in confectione carbonum pleraque pars ſalis acidi, vel communiter ita appellati ſpiritus lignorum, in aerem aufugerit, quæ tamen neceſſariò ad conſtitutionem Salis Alcalino-fixi, tanquam primarium ingrediens, requiritur.

Conſideratu dignum eſt, quod pro acquiſitione cineris vegetabilis abſolute admiſſio aperti vel liberi aeris requiratur, quippe ſine quo nulla ratione cineres acquirantur, unde carbones, occluſo aere, etiam fortiſſimo & per aliquot annos conſtanter durante igne, ſemper carbones permanent, & nunquam in cineres rediguntur.

Nec minus notatu & obſervatu dignum eſt, quod inter carbones & ex eodem ligno confectos cineres, tanta occurrat differentia.

1. Sic ex carbone, quantacunque etiam aquæ copia, ne hilum quidem ſalis Alkalini elixiviari vel ſeparari poteſt.

2. Nullum

2. Nullum ne quidem fortiſſimum & potentiſſimum acidum ullum effectum in carbone exerit.
3. Nullus carbo cum vitro ſeſe miſcet, quæ ſingula tamen cineres præſtant.

Sed ut ad formationem Salis Alcalini, ſeu neceſſaria ad id conſtituentia revertamur, veritas & probatio meæ Theſeos ex pluribus adhuc momentis & circumſtantiis ulterius ſatis patet.

1. Recipiatur ejuſmodi putridum, carioſum & quaſi emortuum lignum, quod alias dum in decenti vigore adhuc exiſtit, largam ſatis lixivioſi ſalis quantitatem edere ſolet ; comburatur, elixivietur, & procedatur in omnibus eadem ratione, ac ſi ſanum lignum fuiſſet, & tunc tandem reſpiciatur, & computus ducatur, quam parum vel nihil Salis Alcali-fixi exinde proveniet. Ratio eſt manifeſta, quia neceſſario ad ejus formationem requiſitæ, tam oleoſæ, quam præprimis quoque acidæ ſalinæ partes antea in aerem exhalarunt & evanuerunt.
2. Aſſumatur planta recens, quæ manifeſto ſufficiente ſalis acidi quantitate à natura dotata eſt, ſeparetur exinde ſal acidum, ſeu ita dictum ſal eſſentiale, quo facto herba exſiccetur, (ſeparato nimirum ante dicto ſale acido) comburatur & elixivietur cum omni cautela, tunc ad oculum patebit, quam parum vel nihil Salis Alcalino-fixi ſuppeditatura ſit.
3. Eadem autem herba, eademque quantitate aſſumta, abſque tamen ſalis acidi vel eſſentialis ſeparatione, tantum ſiccata, combuſta, & modo conſueto ita elixiviata, viſibiliter & aperte per auctiorem Salis Alcali fixi quantitatem, in comparatione

paratione prioris operationis, veritatem demonſtrabit.

4. Vel accipiatur ante extractum ſal eſſentiale acidum, calcinetur debita cum cautione, tum eadem certitudo emerget, & ex eo quoque, quia ejuſmodi ſalia eſſentialia non pure ſalia acida, ſed etiam aperte oleoſis & ad formationem Alcali altera ex parte neceſſariis partibus dotata ſunt, verum ſal Alcali fixum apparatur.

5. Quod ſi porro eadem recentis ejuſmodi herbæ quantitas, quæ ordinario bonam ſatis Salis Alcalini portionem dare ſolet, aſſumatur, factaque ponderatione tota quantitas in duas partes accurate dividatur, ejuſque pars dimidia ſiccetur, comburatur, & modo conſueto Sal Alcali fixum porro exinde elixivietur, ponderetur & conſervetur; altera vero pars dimidia ad putrefaciendum reponatur, & decenter tractetur, quo penitus penituſque putreſcat, tum deſtilletur, ita acquiremus, prout non incognitum eſt, ſpiritum urinoſum, ſal volatile & oleum empyreumaticum; ſed reſpiciatur poſtea, quantum Sal Alkali fixi ex reſiduo jam extrahi poſſit, ſi quidem certo, licet ſumma cum cautione elixivietur, perparum & ſæpe plane nihil emerget; ratio iterum non obſcura eſt, quia requiſita ad fixum Sal Alkali, ſal acidum & oleum, durante putrefactione metamorphoſin paſſa, & partim in ſal volatile, vel ſpiritum urinoſum converſa, partim etiam, & quidem pleraque ex parte, in oleum in ſubſtantia & forma olei per deſtillationem inde ſeparata fuerunt.

6. Quod ſi ad manus ſit purus citri ſuccus, qui aperte ſal acidum, aut paucas vero oleoſas, vel de principio inflammabili participantes partes

ſimul

ſimul continet, ejuſque magna ſatis portio, ad 20, 30, vel plures congios aſſumatur, aquoſum evaporetur, inſpiſſetur, comburatur & elixivietur, tum exinde quoque formatio Salis Alcali fixi manifeſta reddetur.

7. Quam optime autem ſenſibus noſtris hæc patebunt in tartaro, vel ejus cryſtallis quoque, quippe qui, prout unicuique notum eſt, nihil aliud, quam ſal acidum permultis oleoſis partibus imbutum, continet; quod ſi ergo hæc duo hic conjuncta requiſita primaria, nempe ſal acidum & oleum, mediante neceſſario ad hoc inſtrumento, ignis ſcilicet, uſtulentur & comburantur, tum ex reſiduo nullum amplius apertum ſal acidum, è contra vero de novo utique productum Sal Alcali, & quidem copioſe acquiritur; & ſic cum omnibus ſubjectis demonſtrari poteſt.

Quod vero oleoſa quoque ſubſtantia non parum de ſuo contribuat, inter alias diverſas demonſtrationes, (quas, niſi de induſtria nimiam in hac re prolixitatem evitarem, adducere poſſem) etiam ex unica hac cuique facile in ſenſus cadente circumſtantia colligi poteſt; quod ſi *ex. gr.* quatuor libræ ſiccati abſinthii incinerentur, & ex lixivio Sal fixum colligatur, accipimus præter propter 2 ½ uncias Salis Alcali fixi. Jam vero recipiantur itidem 4. libræ ejuſdem abſinthii, contundantur, & conſcindantur eæ, & pars ipſius oleoſa bituminoſa, ſeu reſinoſa, per ſpiritum vini rectificatiſſimum, quamdiu ſcilicet ſpiritus tingitur, extrahatur; tum plurimum quidem, non tamen omne & univerſum in eo contentum inflammabile educes, omne tamen ſalinum in ipſo interim remanet; quod reſtat poſtea, exſiccetur, comburatur, ſal exinde elixivietur & ponderetur, tum loco ante dictarum 2 ½ unciarum

ciarum jam unica tantummodo ſalis uncia prodibit; ex quo ergo iterum apertiſſime neceſſarium, ſub priori combuſtione relictum, & ad majorem Salis Alcalini fixi portionem contribuens, in poſteriori vero antiproba, mediante extractione ſpirituoſa ſejunctum, & jam proinde abſens, ingrediens bituminoſum ſeu oleoſum demonſtratur. Pro ulteriori demonſtratione, hæc jam memorata, ab extractione ſpirituoſa reſidua, remanentia per aquam excoquatur, herba tum reſtans exſiccetur, comburatur, elixivietur, ejuſque ſal colligatur, tum ejus tantummodo 19, vel 20 grana acquirentur; ratio eſt, quia ex his 4. herbæ libris jam omne fere oleoſum & ſal acidum, quæ ad compoſitionem & formationem Salis Alcalino-fixi requiruntur, extractum fuit.

IV. *Quomodo Salia Alcalino-fixa oriantur, & ex quo conſiſtant?*

Cum ergo clare ſatis pateat, quod ad formationem Salis Alcalino fixi acidum & principium quoddam inflammabile abſolute requirantur, prout comprobavi, pro primo facile exinde quoque colligi poteſt, quod aliquid ex his 2. tam neceſſariis ingredientibus ſimul in eo reſtare debeat, prout & revera reſtat.

Ignis, qua inſtrumentum, quod ad confectionem Salis Alcali adhibendum eſt, nihil aliud peragit, quam quod,

1. Omnem ſuperfluam aquam,
2. Omne itidem ſuperfluum ſal acidum in forma ſpiritus,
3. Volatile demum & ſuperfluum oleum ſeparet.

Quemadmodum igitur omnia ſalia, ſive acida, ſive alcalina ſint, ſeu quoquo modo appellentur, nec minus

nus omnia inflammabilia *terram pro baſi vel fundamento* agnoſcunt, quæ terra imprimis oculis ſeſe ſiſtit, ſi ejuſmodi mixta magis magiſque à connexis cum ea volatilibus partibus liberantur, & contra ad magis magiſque fixum ſtatum reducuntur: ita hoc quoque hic in generatione vel compoſitione Salis Alcalino-fixi videre licet; dum enim ignis ſenſim ſenſimque aliquid de volatiliori inflammabili & ſale acido cum aqua propulit, uno eodemque tempore, reſiduum magis fixum tam ſal acidum, quam bituminoſum magis magiſque concentratur, quando ſenſim ſenſimque neceſſaria ad conſtitutionem ſalis acidi aqua perit, quæ mediante calore ignis, ex ſale acido liberatur, & exhalare cogitur; eodem, inquam, tempore ſeparatur, altera ex parte, terra, quæ in ſale acido fuit; quo diutius ignis continuatur, vel ſubjectum incineratur, eo plus aquæ ſubtilis, ſubtilis acidi & inflammabilis abigitur, & tanto plus altera ex parte terræ acquiritur, vel tanto facilius degenerat ſal acidum cum pauca phlogiſti portione, in terram: Unde *maximum volumen ſalis fixi ex pura terra conſiſtit*: verum quia hæc terra originem ſuam ex ſale acido educit, &, ut ita loquar, naturæ acidæ eſt, impoſſibile eſt, quod etiam ſub prima incineratione vel calcinatione omne ſal acidum ex ipſa liberari poſſit; ſed ultimum potius maxime concentratum &, in comparatione prioris multo volatilioris, jam maxime fixum, ut & magis fixum ſubſtantiæ inflammabilis in quavis particula hujus terræ impactum reſtat & inhæret. Salinum hoc cauſa exiſtit, quare omnis hæc terra diſpoſitionem *ſolubilitatis* cum aqua retinet, & paucum illud, quod ex inflammabili ortum deducit, cauſa hic quoque eſt *cauſticitatis*, ſecundum proportionem & modum immixtionis ſui.

Ex eodem quoque fundamento, quia ſcilicet hæc terra acidæ naturæ & familiæ eſt, prodit quoque con-

ſtans quaſi deſiderium, ſal acidum rurſus aſſumendi, quod ipſorum *affinitas & intima commixtio cum omnibus acidis*, quin imo nuperrime à me demonſtratus magnetiſmus, quo Salia Alcalino fixa acidum quoque ex aere attrahunt, tanto magis confirmat. Hoc quoque concentratum & reſtans paucum ſal acidum, ut & hæc ad ſalinam mixtionem prona & jam ab omni aqua liberata terra, aperte unica exiſtit cauſa, quod hæc mixta id rurſus quaſi concupiſcant, quod per calorem ignis amiſerunt, nempe aquam: inde oritur celeris illa & ſubitanea extractio humiditatis & *ſolutio per deliquium*, quin & *incaleſcentia* illa, quod ſi fortiter combuſta fuere, & tum prima vice humiditatem ſentiunt, quin etiam ipſa *incenſio*, ſi minerale quid ſimul adhibitum fuit, quale quid in *ſale Tartari cauſtico cum Regulo Antimonii Martiali* præparato, ut & *pyrophyris* videre licet.

Quod ſi vero Salibus Alcalicis paucum hoc ſal acidum & inflammabile plane adimitur, tum *nuda*, tantum reſtat *terra*, quæ nec in aqua amplius ſolvi poteſt, nec humiditatem nec acidum ex aëre attrahit, nec incaleſcentiam producit, & in omnibus quaſi emortua apparet; quamquam Chymicus, dummodo ipſi lubeat, eam rurſus, [ut ita dicere liceat] revivificare, & ad diſpoſitionem ſolutionis & mixtionis cum aqua, imo ad priorem quaſque priores qualitates reducere valet.

Omne quicquid de formatione Salis Alcalino-fixi ſuperius attuli, unico exemplo aliquatenus jam ſupra memorato plenarie demonſtrare poſſum, vel quivis illud in propriam ſui convictionem imitari poterit: Recipiatur ſal eſſentiale Acetoſellæ, quod viſu, guſtu, tactu, vel quoque modo ante examinatum, [quamvis nemo quoque iſtud negaturus ſit] apertum ſal acidum, non tamen purum, ſed particulis oleoſis mixtum, ſiſtit.

Quod

Quod ſi ergo hujus *e. g.* uncia 1. aſſumatur, ea cum aqua uniatur, tota ſeſe penitus in aqua ſolvit, prout etiam ex ſolutione aquoſa in cryſtallos antea concrevit, & ne minimum quidem indiſſolutum vel cum aqua permixtum reſtat, ita ut nec oleum, nec terra, nec Sal Alcali, nec aliud quicquam peregrinum, ſed nihil aliud, quam pura ſalis acidi ſolutio videri poſſit. Jam recipiatur porro 1. uncia ejuſdem ſicci tamen & cryſtallini ſalis, imponatur ea vaſculo retorto vitreo, & debita cum cautela, igne per gradus adhibito, deſtilletur, tunc ſenſim ſenſimque omne iſtud, quod antea in ſuo ordine circa confectionem Salis Alcali recenſui, jam hic mediante ignis inſtrumento, quod nihil aliud quam vaſculi retorti & contenti in eo ſalis incaleſcentiam cauſatur, eveniet & producetur.

1. In vas recipiens ſuperflua, quæ tamen in ſicco ſale antea latitarunt, aqua & oleum cum ſubtiliſſimo & volatiliſſimo ſale acido prodibunt.
2. E contra non parum terræ inde rurſus ſeparabitur, & reſiduum, cui aliquid ſalis concentrati & olei quaſi fixioris ſeſe innexuit, in perfectum Sal Alcali fixum degenerabit.

Invenientur circiter 3. drachmæ aquæ pauco ſale acido volatili imbutæ, qui liquor alias, ſpiritus acidus Acetoſellæ vocari poteſt, ſcrupulum forte unum olei empyreumatici ponderabis, & in vaſculo retorto, ita dictum reſtans caput mortuum conficiet circiter 4 drachmas & 20 & aliquot grana, ex quo plus quam 3 drachmas veri ſicci Salis Alcali fixi elicies; ita ut pauca tantum grana exhalarint & diſperſa ſint, quæ de tota uncia, ſi ex omnibus computum traxeris, forte deficiant. Hic ergo omnia prodeunt & eveniunt, quæ

 dixi;

dixi; verum præter Sal Alcali &, quæ primo abiit, aquam & ſubtile acidum, oleum quoque adeſt, ut terra plane emortua, quæ duo poſteriora jam nullo modo cum aqua miſceri amplius volunt, quamvis antea, cum adhuc totum ſal acidum oleoſum conſtitueret, omnia in eodem ſolverentur.

Poteſt quoque Sal Alcalino-fixum deſtrui, parva illa acidi & inflammabilis portio ipſi adimi, indeque quantitas terræ augeri, & ſic unico hoc experimento omnium harum rerum certitudo erui.

Conſiſtit itaque Sal Alkali fixum ex terra quadam ſolubili, in quam, mediante igne, aliquid de fixiſſimo ſale acido ſeſe inſinuavit, ſimul quoque aliquid de magis fixo, vel ad terreſtreitatem magis inclinante bituminoſo,ſeu inflammabili principio, uſtulando quaſi innexum eſt, ita ut novum mixtum ſalinum, quod Chymici *Sal Alcali fixum* vocant, conſtituat.

Dominus *Homberg*, qui [prout Domino *D. Stahl* memoratum eſt] primum Pharmacopœus, poſtea vero [ut non nemini notum eſt] clariſſimus Chymicus *Lutetiis Pariſiorum* fuit, quem *Gallia* ad ipſius uſque tempus unquam habuit, hac in re, quamvis revera vir egregie habilis fuerit, tamen errorem paſſus eſt, dum ſal acidum tantum cognovit, quod ſeſe terreſtribus particulis innecteret, & ſic Sal Alcali fixum conſtitueret, de inflammabili vero nullam mentionem injecit.

Qui adhuc vivunt Senior Dominus *Geoffroi* & Dominus *Lemery*, tanquam duo præcipui hodierni *Galliæ* Chymici, in tantum quidem ſub ſuis laboribus obſervarunt, quod aliud quippiam adhuc præter ſupra dictum ſal acidum & terram, nempe aliquid de principio inflammabili ad conſtitutionem Salis Alcali neceſſario requiratur; ſed boni iſti viri rurſus in eo defecerunt, dum hoc inſtrumento, ſeu igni, aſſignare

voluerunt, cum tamen in iſto ad Sal Alcali fixum adhibito ſubjecto vegetabili, reale ac ſubſtantiale ingrediens oleoſum vel bituminoſum uniuſcujuſque oculis obverſetur. Porrigatur quæſo his Dominis ejuſmodi ſubjectum in manus, in quo antea nullæ actuales partes inflammabiles demonſtrari vel conjecturari poſſunt, ſed in quo tantum ſal acidum & partes terreſtres probari queunt; hoc ſubjectum ipſi poterunt in vaſe retorto, per 20 & plures annos, cum multis centenariis extrorſum vaſi retorto adhibitis, particularum ignis & materiarum luminis [*Parties de feu, & matieres de la lumiere*] vel qua ratione ipſi volunt tractare, ea tamen cum expreſſa præcautione, ne cineres ipſos harum adhibitarum inſtrumentalium materiarum ignis addant, dum certo certius ſit ipſos ne unicum quidem granum Salis Alcalino-fixi exinde philoſophando educturos, licet etiam totum *Carteſium* ſimul igni conſecrarint: jam vero ſi ex ipſorum opinione ignis ingrediens, vel conſtituens materiale ipſius exiſteret, neceſſum foret, ut abſolute etiam ex ejuſmodi rebus, in quibus in ſe nullæ eſſentiales partes inflammabiles præſto & immixtæ eſſent, illud conficere poſſint: Quod ſi in actum deducere non poterunt, prout revera non poterunt, ſed ſemper materia in & ex ſe ipſo inflammabilis aſſumenda venit, quare nòn ipſas actu realiter præſentes adhibitas, plane certas & demonſtrativas res, tanquam cauſam reſpiciam, vel quare memet huic & illi Philoſopho vel Chymico cerebrino, qui forte per totam ſuam vitam nullum carbonem manibus tractavit, & chymica ſubjecta & hiſtorias non ex ipſorum intima qualitate, & realiter conſtituentibus materialibus partibus diverſarum in iis contentarum materiarum, ſed tantummodo ab extera, mediante circulo, linea, & microſcopio, vel etiam quæ cum iis dimenſurare & perſpicere

perſpicere non poteſt, ſecundum propriam chymericam in cerebro tantummodo propoſitam, & in Chymico laboratorio nunquam comprobatam, nec unquam comprobandam imaginationem probabile judicat, ſubjiciam, ipſiuſque deliramenta adorem?

Quod autem ignitio, vel ignis, qua inſtrumentum tantum, ad productionem Salis Alcali fixi requiratur, inter cætera ex Tartaro quoque patet, quippe qui utraque primaria præſidia poſſidet. Poſſidet, ut notum eſt, partes oleoſo bituminoſas, continet etiam apertum ſal acidum, & utrumque in abundantia; quod ſi autem ex hoc Sal Alkali abſque igne artificioſe deberet produci, tum nihil amplius adhibendum foret, quam volumen, vel terra quædam, in quam parum acidi & inflammabilis ſeſe innecteret, & innidularetur; proinde pro hoc ſcopo oculos Cancri, vel aliud corpus terreum, quod ad minimum cum nullo alio acido imbutum eſſet, adhibere poſſent. Solve igitur in Tartari ſolutione, tantum ante dicti terrei corporis, quantum ſolvi poteſt, evapora aquam & exſicca, tum equidem ex propria mea hypotheſi, qua dixi, quod ſal acidum & inflammabile in terra concentrata abſolvant Sal Alcali fixum, vel quod Sal Alcali fixum ex copioſiore terra & pauciore ſale acido & inflammabili fixiore conſiſtat, poſſibilitatem & mixtum quoddam, quod in pluribus circumſtantiis ut Alcali fixum ſeſe gerit, oculis ſiſtes; verum examinetur ulterius ſecundum mones à me allatas probationes veri & perfecti Salis Alcalino-fixi, tum mox hic, mox alibi patebit, multa adhuc illi deeſſe, & quomodo totum mixtum, ſolutio tantum oculorum cancrorum in acido Tartari, adeoque ſuperfluum acidum & oleum, breviter ipſe Tartarus crudus adhuc ſimul in illo immerſus & irretitus ſit, per conſequens nullo modo verum & perfectum Sal Alcali fixum conſtituat, nulla alia ex ratione,

quam quia nullus ignis adhibitus fuit, non ac ſi ex igne plures adhuc particulæ ignis ad hoc neceſſariæ eſſent, ſiquidem, prout dixi, hic adhuc ſuperfluæ inflammabiles partes in oleo Tartari præſentes ſunt, ſed propterea quia ſuperfluum oleum & acidum exinde ſeparari debuiſſent, unde etiam in hoc experimento, formatio Salis Alcali ad oculum patet, ſimulac modo plus acidi & Alcali per adhibitum calorem ſenſim ſenſimque expulſum, & operationi per neceſſarium inſtrumentum colophon impoſitus fuit.

Induſtrius olim *Kunckelius* jam tum temporis animadvertit, quod ex Tartaro & Calce viva, ſi utraque cum aqua coquendo elixivientur, poſſibilis demonſtratio Salis-Alcalini, abſque adhibita ſicca ignitione demonſtrari poſſit; cum vero cum præcedentibus circiter conveniat, & eadem ratione quædam partes olei empyreumatici Tartari adhuc aliquatenus reſtent, ſuperfluum exiſtimo iſtud in meæ hypotheſeos probationem adducere, prout & alias non paucas adhuc cum variis acidis imprægnatas terras, vel plenarias terrarum ſolutiones, tanquam falſa Salia Alcalino fixa adducere poſſem, niſi prolixitati parcerem.

V. *De Salibus Alcalino-fixis maxime uſualibus, & quomodo paranda ſint?*

Equidem ſatis ſuperque notum eſt, quod ex omnibus vegetabilibus, vel potius eorum cineribus Sal Alcali parari poſſit, nec non quod ex permultis herbis & permultis aliis partibus à non exiguo temporis ſpatio ejuſmodi lixivioſa Salia confecta fuerint, & denique quod eodem modo omnia conficiantur, nihilo minus quædam adhuc de hac materia, tam in genere de Salibus fixis herbarum, quam etiam in ſpecie de reſiduis adhuc in Chymia uſualibus Salibus Alcalino-fixis, ſeu

ſeu potius eorum præparatione, quantum fieri poteſt, brevibus adducam.

Modus procedendi ab aliis in 9, ab aliis vero in 6 puncta diſtribuitur; qui in novem puncta ipſum diſpertiuntur, appellant operationes vel labores, qui pro hoc ſcopo cum ſubjecto vegetabili perficiuntur, prout ex ordine ſequuntur, deſiccationem, combuſtionem, extractionem, ſeparationem, inſpiſſationem, calcinationem, ſolutionem, filtrationem & inſpiſſationem.

Qui vero 6. puncta tantum requirunt, appellant hos labores, deſiccationem, combuſtionem, extractionem, ſeparationem, inſpiſſationem & depurationem.

Ego ad quatuor eas reduco, nimirum *incinerationem*, *elixiviationem*, *inſpiſſationem & depurationem*, dum ſcilicet unus labor alterum ſemper præſupponit; quod ſi enim *de incineratione* loquor, quod nempe vegetabile primo in cineres exuri debeat, tum hoc jam præſupponit deſiccationem, vel quod antea, ſi non ex toto, ſaltem aliqua ex parte exſiccari debeat. Si de ſecundo puncto *elixiviatione* ſcilicet loquor, vel ipſæ anus norunt, quod nullum lixivium abſque aqua vel extractione fieri poſſit; norunt quoque, quod hæc *extractio*, ſi lixivium appellari debet, à cineribus ſeparanda ſit, ut proinde elixiviatio extractionem & ſeparationem utique præſupponat; & ſic etiam ſub operationibus *depurationis*, neceſſariæ ad eam calcinationes, ſolutiones, filtrationes & inſpiſſationes intelligendæ veniunt.

Totus proceſſus producendi Salia Alcalia fixa ex herbis, ſequentibus abſolvitur momentis, quæ breviter percurremus: Recipitur herba ejuſmodi, eaque leniter ſiccatur; partem aliquam ſiccatæ herbæ de alio igne incende, in ignem ſenſim ſenſimque reliquum herbarum conſtanter projice, ita ut leni tantum carbonum igne, minime vero aperta flamma, exuratur &

in

in cineres redigatur, cui rei non parum favet, ſi hic carbonum ignis circa ultimum in magis magiſque anguſtum ſemper acervum colligatur. Quod ſi tandem penitus exuſtio peracta eſt, refrigeſcat, cinis cribretur, ut reſtantes forte non plane incineratæ particulæ ſeparentur, cinis aqua frigida plenarie elixivietur, lixivium filtretur, filtratum vel in figulino, vel vitreo vaſe leniter evaporetur, denique ad ſiccitatem inſpiſſetur.

Medico uſui ſi adaptari debent, & ſi Medicus externam plane non neceſſariam, imo potius noxiam magis quam utilem, pulchritudinem & candorem reſpicere non vult, tum prima hæc inſpiſſatio ſufficere poterit; quod ſi vero depurari, imo ex parte plane cryſtalliſari debent, tum calcinantur parum, & denuo in magna ſatis portione aquæ ſolvuntur, filtrantur, rurſuſque leniter evaporantur eouſque, donec in ſuperficie parva cuticula apparere incipiat; tum enim in fundum detruncatæ cucurbitæ, vel aliud ampli orificii vitrum infundatur, & impoſita leviter charta a cinere vel aliis ſordibus præcaveatur, & in locum temperate calidum reponatur, tum ſenſim ſenſimque magna pars in pulchras & egregias cryſtallos jucundo ſæpe ſpectaculo vertetur, præcipue ſi lixivium, adhuc ante apparitionem cuticulæ, ab igne auferatur, & in locum leniſſime calidum ad cryſtalliſandum reponatur, & abſque ulla motitatione ſatis temporis ipſi concedatur. Poſtquam igitur in cryſtallos concrevit, reſtans lixivium ab iis decantetur, pars humiditatis rurſus leni igne exhaletur, rurſus in ante dictum temperatum locum reponatur, ut reſiduum, quantum fieri poteſt, cryſtalliſet, quæ tamen cryſtalli plerumque pulchritudine ad priores non accedunt, ſed multo minores, imo plane aliter formatæ eſſe ſolent. Quod tum vero ſupereſt liquidum,

decantetur

decantetur, & tandem plenarie inſpiſſetur; cryſtalli autem ſemper pauca aqua deſtillata communi celeriter abluantur, & tum ſuper pura charta bibula ab adhærente humiditate leniter ſiccentur: Si vero non cryſtallos, ſed tantum depuratum album Sal Alcali deſideres, ante dicto diuturno labore ſuperſedere, & tantum poſt primam calcinationem, ſolutionem & filtrationem, ad ſiccitatem leniter inſpiſſare, & tum in obturato firmiter vitro ab aëre bene præmunire poteris. Hæc igitur eſt generalis methodus, qua ratione ex omnibus herbis, quæ in Pharmacopoliis et in Medicina adhuc uſitata ſunt, ſalia lixivioſa herbarum, florum, ſtipitum *&c.* puriſſime & optime præparari poſſunt, ubi tria adhuc monenda erunt.

1. Simulac una tantum, vel altera levis circumſtantia in ante dicto proceſſu immutatur, ſive ſit in exurendo, in elixiviando, evaporando, calcinando, ſive quicquid ejus rei ſit, quod ſtatim et infallibiliter modo hic, modo alibi immutatum ſal acquiſiturus ſis.

2. Quod in ejuſmodi depuratis et cryſtalliſatis Salibus Lixivioſis nulla, quæ in ſubjecto antea fuit, ſpecifica virtus & proprietas demonſtrari queat.

3. Quod omnia uſitata & præcipue cryſtalliſata Salia herbarum nunquam pura Salia Alcalinofixa ſint, vel qua talia ad alios Chymicos labores, in quibus purum Sal Alcali fixum requiritur, adhiberi debeant.

Tachenius

Tachenius et cum eo alii contendunt, herbas recentes plus ſalis largiri quam ſiccatas, *Rivinus* autem id pernegat, et ait, ſe id omni adhibita diligentia obſervaſſe, & de eadem herba probam ſumſiſſe; ſed nullum diſcrimen in iis potuiſſe reperire. Meo quidem judicio uterque certo modo verum dixit: Nam ſi ejuſmodi vegetabile tractes, quod nullum ſubtile vel volatile ſal acidum continet, tum ex ſiccato idem pondus ſalis ac ex recenti acquires: vel etiam ſi herbam recentem, aperto igne flammæ, ſiccatam contra leni tantum candefactionis igne incineres, itidem ſemper idem fere aut ſaltem parum differens pondus, quamvis non ejuſdem indolis, Salis Alcalini emerget, adeoque hac ratione *Rivini* quoque opinio veritati conſona eſt.

Sed ſi ejuſmodi vegetabile ſub manus ſit, quod vel tenerum, ſubtile, vel etiam cum aliis partibus obiter tantum commixtum ſal acidum continet, quod ſub incineratione facillime abſcedit, exhalat, evaporat & aufugit; tum utique recens aut ſaltem non adeo plene exſiccatum multo plus Salis largietur, quam aliud, licet tamen unam vel alteram adhuc enchireſin requirat, et hac ratione *Tachenius* applauſum meretur.

Dantur vegetabilia, quæ omni licet induſtria et præcautione ſub incineratione adhibita, tamen ſubtile ſuum et ad fabricam Salis Alcalici neceſſarium ſal acidum dimittunt, ita ut ex magna ſatis quantitate perparum tamen Salis Alcalici fixi acquiratur. Quod ſi autem exhalatio ſalis acidi hoc vel alio modo impediatur, tum ad oculum plus Salis Alcalini fixi prodibit, id quod rurſus ea, quæ de generatione et formatione horum ſalium dixi, luculenter confirmat.

Hoc quo eo magis pateſcat, recipiatur tantum *Lignum Guajacum*, vel *Sanctum*, quippe quod ejuſmodi ſubjectum exiſtit, quod ſub deflagratione ſal ſuum

acidum perquam facile dimittit, quod ſi ſatis magna ejus quantitas exuratur, incineretur, & ex cineribus omni cum præcautione Sal Alkali colligatur, perquam *exigua* ipſius *quantitas* erit ſecundum proportionem aſſumpti ligni; cujus rei rationem modo attuli, quia ſcilicet ſubtile acidum, ſub deflagratione & effumatione plurima ex parte diffugit; quod ſi autem eadem ligni quantitas in minutiſſima fruſtula concidatur, vel plane raſpetur, & hoc cum magna aquæ quantitate excoquatur, decoctum coletur, reſiduo ligno recens aqua affundatur, & excoctio eo uſque continuetur, donec aqua non amplius tingatur, decocta omnia confundantur, evaporentur, & in extractum inſpiſſentur, quod leniter exſiccetur, & tum tam hoc quam reſidua ſeorſim ſiccata raſpatura vel fruſtula leniter candefaciendo exurantur & incinerentur, tum elixivientur, & methodo conſueta Sal Alkali ex iis colligatur, mirum quanto *major* ipſius *quantitas* inde proditura ſit, quod & de non paucis aliis valet.

Præter allata Salia Alcalino-fixa herbarum, proſtat etiam Alcali quoddam lignorum minime pretioſum, & ad multas operationes non incommodum, hoc, quamvis jam plene elixiviatum, inſpiſſatum & calcinatum Sal Alcali revera exiſtat, tamen communiter tam Latina, quam Germanica, ut & quibuſdam aliis linguis, *cinis* tantum appellatur, *cineres clavellatos* indigito. Quicquid ſit, ſive olim purus non elixatus cinis ſub hoc nomine venditus, indeque denominatio retenta fuerit, ſive alia cauſa fuerit, originem hujus nominis inquirere operæ pretium eſſe vix exiſtimo. Interim cineres clavellati unicuique ſatis noti ſunt, et a quibuſdam ſimpliciter *Sal Alcali* appellantur. Præparantur communiter ex cineribus lignorum duriorum, fagi, aceris, betulæ, fraxini, quercus & ſimilium, unde & quomodo parentur, & qua

qua ratione optimi cognoſcuntur, videre licet in *Kunckelii Arte Vitriaria*, *Valentini Muſæo Muſæorum*, & in Tractatulo, qui *Ars tinctoria experimentalis & fundamentalis* inſignitur.

Hi cineres clavellati prout emuntur & venduntur, *non* conſtituunt plane *purum* & ad omnes labores chymicos aptum *Sal Alcali*, quapropter ii antea adhuc ſemel depurandi ſunt, quod non difficulter fieri poteſt, ſiquidem nihil aliud agendum, quam ut dictis cineribus clavellatis *aqua* & quidem *frigida*, ſed ne nimia copia, affundatur, ſolvantur, ſolutio filtretur, leniter evaporetur & inſpiſſetur, tum eadem ratione & bonitate ac quodvis *puriſſimum Sal Alcali* adhiberi poterit. Id quod a decantata ſolutione cinerum clavellatorum reſtat, & in aqua frigida ſolvi negat, *non* eſt amplius *purum Sal Alcali* fixum, ſed præter partes alcalinas adhuc etiam de *Sale acido*, & quidem *vitriolico*, participat : nam ſi iſtud aqua fervide calente plene diſſolvatur, filtretur, & ſolutio, poſtquam ex parte evaporata fuit, per aliquot horas reponatur, candidas & egregias cryſtallos reperies, quæ in omnibus probis & inquiſitionibus ut verum ſal enixum ſeu medium, quod ex alcali fixo & acido Vitriolico componitur, breviter ut *Tartarus vitriolatus* ſeſe gerant ; quod autem ab hac ſecunda cinerum clavellatorum cum aqua fervida tractatione ſolvi recuſat, & in filtro remanet, ſunt inutiles plane fæces & ſordes.

Præterea extat aliud purum Sal Alcalino-fixum, quod ordinario pro optimo habetur, & hactenus tam ad Chymiam, quam ad Medicinam præ omnibus aliis adhibitum fuit ; eſt iſtud *Sal Tartari* ; qua ratione id paretur, itidem res eſt ſatis ſuperque nota, qui vero illud tenerius & penetrantius requirit, duplici modo hoc adipiſci poterit, *vel calcinando, ſolvendo, filtrando & inſpiſſando, vel etiam calcinando, per deliquium ſolvendo, filtrando*

filtrando et inſpiſſando ; hi duo purificandi modi, ſi non ſæpiuſcule reiterentur, in pleriſque conveniunt, & tum *differunt tantum in ſolvendo & deliqueſcendo:* quod ſi autem una ex parte *calcinatio*, et altera ex parte *deliqueſcentia* ſæpe ſæpiuſque inſtituatur & reiteretur, tum ingens utique & ſat *notabile diſcrimen* oſtendent.

1. Inſtituta & reiterata ſæpe *calcinatione* ſenſim ſenſimque aliquid de reſtante adhuc inflammabili & ſale acido, imo & de ſubtiliſſima terreſtri parte ipſa deſtruitur, imo totum mixtum Sal Alkali conſtituens quaſi invertitur,& in puram in aqua non amplius diſſolubilem *Terram* commutatur: quod ſi ſupra adductorum Autorum opinio vera exiſteret, & *particulæ ignis* ad productionem Salis Alkali,*tanquam materiale ingrediens*, concurrerent, hic plane non ad ejuſmodi deſtructionem, ſed potius ad augmentationem Salis Alkali fixi devenire deberet, dum hac ſæpius repetita calcinatione *recentes* ſemper *particulæ ignis* admoventur, tantum abeſt, ut eæ deficere poſſint ; nihilominus unuſquiſque veritatem dictorum experiri, deque hac repetita calcinatione experientiam conſulere poterit.

2. E contra, inſtituta & repetita *ſæpius ſolutio per deliquium* & inſpiſſatio plane contrarium demonſtrat, dum ejus vice, quod per calcinationem ſubtilius aufugit, & magis terreſtre remanet, hic potius ejuſmodi crudiores terreſtres particulæ ex Salibus Alcalibus fixis potius ſeparantur, extricantur, *teneriores* contra & *ſubtiliores* in his per deliquium ſolutis ſalibus retinentur, unde ejuſmodi ſalia, hac ſæpe ſæpiuſque repetita per deliquium ſolutione, eo tandem redigi poſſunt, ut *nec in frigore, nec in calore amplius ſiccam conſiſtentiam recuperent, ſed ſemper conſiſtentiam humidam, fluxilem*, forte ut oleum Vitrioli, *conſervent*, & in igne ejuſmodi

ejuſmodi ſal tenuiora crucibula potius ſolvat, craſſiora autem citius penetret, quam in forma ſicca appareat.

Non inuſitatum etiam Sal Alcali fixum eſt ita appellatum *Nitrum fixum*, ſeu Nitrum alcaliſatum: Quod licet cuique notum, ejuſque præparatio tanquam vilis & nauci æſtimari poſſit, meas tamen quoque circa iſtud negotium cogitationes, forte quibuſdam ſcitu adhuc neceſſarias, jam exponam.

Alkaliſatio Nitri ſequenti innititur fundamento: ſimulac *Nitrum cum quodam bituminoſo vel craſſe terreſtri inflammabili corpore miſcetur & detonatur*, ſtatim una ex parte Nitri reſolutio, & contra altera ex parte generatio, vel nova compoſitio Salis Alcali oritur, ubi bene inſimul notandum, quod non tantum Sal Alkali fixum, ſed & in eodem momento *adhuc Sal Alcali volatile, vel urinoſum* generetur, quæ ſingularis metamorphoſis attento Chymico non tantum nudam & gratam admirationem cauſatur, ſed & ſæpius revera ad manu palpabilem, ut ita dicam, cognitionem imperſcrutabilis omnipotentiæ Dei, & maxime ſubmiſſam venerationem mirabilium ſuorum operum devotam mentem conducit.

Sed, ut ad propoſitum meum revertar, quod ſi, inquam, Nitrum cum terreſtri quodam inflammabili corpore commiſcetur, exempli gratia, bitumine, pice, colophonio, fuligine, tartaro, maſtiche, ſulphure communi, carbonibus, gummi ſandarachæ & ſimilibus, tum Alkaliſatio ſtatim procedit, ſimulac cum iis detonatur, vel deflagrat; ſed cum *ſubtilioribus* mixtis *inflammabilibus*, ut ſpiritu vini rectificatiſſimo, & ipſis oleis deſtillatis, non ita procedit; niſi cum poſterioribus ſingularia artificia adhibeantur. Viliſſimum & proinde etiam uſitatiſſimum additamentum ſunt carbones pulveriſati, ut notum eſt; ne autem hunc tibi conceptum in animo formes, ac ſi Sal Alkali ex adhibitis carbonibus

bus ortum duceret, ſiquidem tota carbonum libra tantum aliquot drachmas cinerum, hi cineres vero tantum aliquot grana Salis Alcali fixi largiuntur, id totum fere a Nitro ipſo dependet, unde quoque pro ulteriori adhuc penſitatione adducam ſequentia momenta. Libra una optime depurati Nitri cryſtallini continet præter propter partem dimidiam aquæ, unam quartam ſalis acidi, & unam quartam additamenti Alcalini, quod ad corporificationem acidi nitroſi, & ad figurationem formæ cryſtallinæ in conficiendo Nitro additur, unde quoque totum Nitrum ſal medium exiſtit; quod ſi autem huic libræ Nitri tres vel quatuor carbonum uncias admiſceas, quæ *per ſe* circiter 3. *vel* 4. *grana Salis Alkali* fixi ſuppeditant, & cum hac carbonum quantitate ante dictam Nitri libram alcaliſes, accipies 8. vel 10. imo, ſi recte operatus fueris, fere 12. uncias purificati & boni Salis Alcalini. Jam ergo oritur quæſtio: *unde tantum pondus?* ſi quidem vix 4. unciæ additamenti alcalini in tota Nitri libra præſto ſunt. Quiſque rem hanc ulterius conſideret, & ea, quæ de origine Salis Alcali fuſe ſatis dixi, in conſilium trahat; forte tum quæſtionem hanc ſibi ipſemet facile diſſolvere poterit.

Dixi equidem in genere, Nitrum cum omnibus craſſe terreſtribus inflammabilibus ſubjectis alcaliſari poſſe; ſed ne cogites proinde velim, ac ſi etiam in genere unum, & quoad omnia ſemper idem, Sal Alkali fixum inde proditurum ſit. Minime; ſed prout inter inflammabilia additamenta ipſa, modo hoc, modo illud diſcrimen reperitur, ita ſemper utique aliquo modo diverſum, modo magis vel minus ſubtile, craſſius, modo ſumme cauſticum, vel etiam obtuſius, ſemper tamen Nitrum fixum, quamvis non ubique & omni ex parte plane æquale, ſed ut dixi, modo hic, modo alibi differens

differens Alcali, quod vel a proportione, vel, & quidem ut plurimum,a differente indole & natura additamenti, ut & ſingulari ratione ab iſto magis magiſque fixo, vel in aliis fixis materiis concentrato, inflammabili dependet, ex quo *cauſticitas* quoque primario *ortum* ſuum *deducit*, *e. g.* ſi nitro ejuſmodi ſubjectum admiſceo, in quo inflammabile equidem copioſe, ſed *tantum ſuperficie tenus* præſto, imo *cum multa aqua* adhuc extenſum, & proinde *magis ad volatilitatem diſpoſitum* eſt, ut *in reſinis*, pice, colophonio & ſimilibus, tum accipio quidem *nitrum fixum*, ſeu alkaliſatum, ſed longe *non adeo cauſticum*, quam ſi ejuſmodi ſubjectum ipſe addo, in quo *phlogiſton magis fixum & terreſtre* factum eſt, quod iterum ex gr. *per additamentum Reguli Antimonii Martialis* facile demonſtrari poteſt ; quippe in quo ſolummodo concentratum principium *inflammabile terreæ indolis* ex Marte *cauſticitatis vera cauſa* exiſtit, qua occaſione non diffiteor, memet ipſum, in quantum adhuc *originem cauſticitatis* conſideravi & obſervavi, huc uſque *nullum Sal Alcali fixum abſque additione ſubjecti mineralis cauſticum reddere noſſe* & poſſe, ut propterea hæc materia ulteriore adhuc diſquiſitione indigeat ; imprimis cum & alia occaſione oſtenderim, qua ratione ex eodem plane fundamento *Sal Alcali volatile cauſticum* produci queat.

Ordinarium *Nitrum cauſticum* fixum ex partibus anaticis Reguli Antimonii Martialis & nitri cryſtalliſati pulveriſatis ante & mixtis cæmentando paratur, prout notum eſt.

Quin & Regulus Antimonii, calx viva, ut & plura alia mineralia tartaro crudo ejuſque ſali, imo etiam ipſis depuratis cineribus clavellatis, methodo ſatis nota *enormem cauſticitatem* inferunt, ita ut ordinarium cum calce viva & depuratis cineribus clavellatis, vel ſale

tartari paratum, & ordinario quoque appellatum *Sal alcali cauſticum* omnia corpora tenacia animalium, ut pennas, crines, lanam. oſſa, cornua, ungulos, *&c.* in *mucum*, & pinguedines parvo temporis ſpatio in *ſaponem*, ſulphur autem in *rubrum liquorem* diſſolvat. Quod ſi hoc Sal in forma liquida ſolutum exiſtat, tum appellatur *lixivium ſaponariorum*; ſi adhuc ſemel in crucibulo fundatur, & tum in exiguas oblongas portiones, præter propter ut partes fiſtulæ tabaci ſimiles, effundatur, tum *lapis infernalis alcalicus*, *cauterium potentiale*, imo & quibus *Sal Chirurgorum* appellatur.

Tandem præter *duas* has a me allegatas magiſque conſuetas *nitrum* alcaliſandi *methodos*, *adhuc tertia* huc illuc uſitata eſt, nimirum, *ſi nitro*, carbonum vel reguli loco, *tartarus pulveriſatus addatur.* Quanquam hoc præparatum, ex uſitata conſuetudine, *nunquam nitrum fixum*, ſed poſtquam elixiviatum & inſpiſſatum fuit, potius *ſal tartari extemporaneum*, & ſi non elixiviatum vel depuratum fuerit apud aurifabros, docimaſtas, metallurgos, monetarum tractatores, zonarios, alioſque circa metalla verſantes artifices, *fluxus niger* appelletur: Verum & hoc nitrum fixum non ita promiſcue pro quovis alio ſumendum erit, ſi quidem itidem *pro diverſa* dictorum duorum ingredientium *proportione*, *etiam diverſa ſalia* emergunt.

Quod ſi igitur *fluxum* hunc *nigrum* præparare volo, recipiuntur 1. vel *partes æquales* utriuſque, vel etiam 2. *nitri pars una & tartari partes duæ vel tres:* ante dicti mechanici artifices ſolent utrumque ſeorſim pulveriſare, pulveres utroſque miſcere, tum in purum ferreum mortarium injicere & pruna candente incendere, & plerumque hoc ita crude adhibent, abſque ulla elixatione, filtratione & inſpiſſatione, ſeu depuratione. 3. Quod ſi vero prima ſtatim detonatione album plane ſal deſidero, tum *ad tres*

vel duas nitri partes tantum unica tartari recipitur.

Circa has tres *diverſas proportiones*, ut ante jam notavi, etiam ſequentes *diverſæ circumſtantiæ* occurrunt.

1. *Si* 2 *partes nitri ad unam tartari partem* aſſumantur, tum *non omne nitrum* plane *alcaliſatum* exiſtit, ſed ex parte adhuc nitrum crudum ei inexiſtit.

2. *Si* 2 *vel* 3 *partes tartari cum* 1 *parte nitri* commiſceantur, tum omne nitrum equidem reſolutum & alcaliſatum eſt, ſed e contra *non omnis oleoſa tartari ſubſtantia conſumpta & exuſta* eſt, ſed magna pars adhuc reſtat, unde quoque idcirco *hæc proportio* tantum & unice ad *liquefactiones* & *reductiones metallorum* præ aliis proportionibus longe commodior, aptior & *utilior* deprehenditur, interim & hoc (ſi volupe fuerit) inſtituta de novo calcinatione, itidem a ſuperfluis oleoſis partibus liberari, & ſic in *perfectum Sal alcali redigi* poteſt, quod vero tum ſal tartari extemporaneum, vel ex tempore paratum, vocari non poteſt.

3. Proportio media, ubi *partes anaticæ* aſſumuntur, optima quidem videtur, interim negari non poteſt & eam *adhuc* quandam *nitri crudi partem* ſecum retinere.

Sub hac modo adducta, & tanto magis *ex* 2 *partibus nitri* compoſita proportione, durante deflagratione, *realis ſeparatio principiorum vel partium conſtituentium tartari oboritur*, ubi 4 *notabiles circumſtantiæ & momenta* occurrunt.

1. Maxima pars oleoſitatis tartari non modo reſolvitur, ſed & ita conſumitur, ut vice ejus quod alias in deſtillatione tartari notabilis *copia olei*

in forma fluida ſeparatur & apparet, *hic* ſub detonatione cum nitro *nullum* aliud *ipſius veſtigium*, quam paucum ſiccæ fuliginis ob oculos veniat.

2. Altera notabilis circumſtantia ſub hac ſalis tartari extemporanei formatione hæc eſt, quod, cum alias durante deſtillatione vel exuſtione tartari, ſeparatio olei, ſpiritus & phlegmatis una ex parte, & nova formatio ſalis fixi alcalini altera ex parte, per conſequens *omnia tarde* eveniant, & aliquod temporis ſpatium requirat, *hic omnia ſimul & ſemel unico momento* & unica detonatione procedunt.

3. Notum eſt quod tartarus & nitrum (quodvis ſeorſim) *ſi deſtillentur*, *ſpiritum acidum* ſuppeditent, *hic autem*, ubi hæc duo ſalina mixta conjunctim deflagrant, *ne hilum* quidem *acidi* emergit, *ſed* rurſus plane aliud, imo *contrarium*, & de novo formatum, *ſcilicet urinoſum* ſenſibus noſtris ſeſe ſiſtit.

4. Quarta non minus notabilis circumſtantia hæc eſt, quod, cum in communi exuſtione tartari per ſe uſque additamento, ſemper *non exigua portio terræ indiſſolubilis* poſt perpetratam Salis Alcalici elixiviationem reſtet, hic in ſalis tartari extemporanei præparatione *fere nihil indiſſolubilis terræ* reperiatur, ſed omne iſtud *in purum ſal*, & quidem adhuc, ut ante jam dixi, *in momento & incomprehenſibili celeritate* convertatur.

Partem reliquam hujus Diſſertationis proxima occaſione communicabimus.

II. *Obſer-*

II. *Obſervations of the Eclipſe of the Moon*, Octob. 10*th*. 1725. *made at* Briſtol, *by* John Burroughs *Eſq*; *communicated in a Letter to Mr.* Cheſelden, Surgeon *to St.* Thomas's *Hoſpital, F. R. S.*

THE cloudy Weather here prevented us from ſeeing the Beginning of the Eclipſe, or of total Darkneſs; but I obſerved, pretty exactly, the firſt Appearance of Light, after the total Darkneſs, and the End of the Eclipſe; and their reſpective Times are as follows, *viz.*

	h	′	″	
Beginning of Light	7	31	20	} apparent Time.
End of the Eclipſe	8	29	30	

Some ſmall Time before the renewal of true Light, there appeared a remarkable Brightneſs upon the Eaſtern Limb of the Moon, which I am not certain how to account for; at firſt I reaſoned with my ſelf in this Manner; That all the Light which the Moon has, during her total Immerſion in the Shadow, is by Rays from the Sun, refracted through the Earth's Atmoſphere, and conſequently thoſe Rays muſt be in greater Plenty, near the Edge of the Shadow, than near the Axis. But this does not fully anſwer the Phænomenon, becauſe the Light I ſpeak of, was not only upon the Moon itſelf, (on whoſe Eaſtern Limb there was a manifeſt increaſe of Light, as ſhe approached the Edge of the Shadow) but it was alſo diffuſed about the Edge of the Moon, to a ſenſible

Diſtance, which I would certainly have meaſured, if I had had proper Inſtruments. And if others, who are more skilled in theſe Affairs, have made the like Obſervation, I ſhall no longer doubt of the Moon's having an Atmoſphere.

Briſtol, Feb. 19.
1725.

III. *Sceletωn duorum Humanorum petrefactorum pars. Ex Epiſtola* Joh. Jac. Scheuczer, *M. D. R. S. S. ad Dom.* Hans Sloane, *Baronettum, S. R. V. Pr.*

NOSTI, Vir Expertiſſime, ex hominum diluvio ſubmerſonum genere paucas ſuperſtites eſſe reliquias. Neque ego hactenus in numeroſa ſatis collectione plures habui quam binas dorſi vertebras, atronitentis ſplendoris, petrefactas. Nunc autem, ἀγαθῇ τύχῃ Muſeolo meo illatum, lapidi fiſſili *Oeningenſi* immerſum λείψανον, omni attentione digniſſimum, in quo diſtincte cernere licet, non vagæ imaginationis ſimulacra, ſed capitis humani à quovis alio animantium genere diſtinctivi partes benè multas, reapſe reſiduas, cranii ambitum, os frontis, oſſa ſincipitis, occipitis, orbitam oculi, baſeos cerebri & medullæ oblongatæ λείψανα, prominentiam interiorem oſſis occipitalis, quæ cerebelli lobos ſeparat, colli vertebras numero 7, partim denudatas, partim lapideo cortice tectas, & eſt hæc veluti orthographica ſectio poſterioris capitis partis.

Sed

Sed inde ex quo Monumenti hujus fui potitus, ſingulari Divinâ Providentiâ novus ex dicta lapicidina advolavit hoſpes, carior tantò, quia eſt ex rariſſimis, charior, quòd nec veſte opus habeat, nec alimento, omnia ſecum portans, priorem vincens & magnitudine, & ætate, & curioſitate. Adſunt nempe in dicto fiſſili lapide demerſa, ex hominis adulti ſceleto, ejuſve ſtructura anteriore, peripheria oſſis frontalis, os jugale, orbitæ oculorum, cranii tabulæ cum diplöe, veſtigia foraminis infraorbitalis deſtinati pro tranſitu nervorum quinti paris, reliquiæ vel ipſius cerebri, vel duræ matris, oſſa cribroſa & ſpongioſa, os vomeris naſum diſterminans, oſſis quarti maxillaris portio, quæ genas conſtituit, naſi reliquiæ, maſſeteris portio, ſectio orthographica tranſiens per apophyſin condyloidem maxillæ inferioris ad angulum uſque hujuſdem maxillæ, vertebræ in continua ſerie numero 16, pleræque cum proceſſibus tranſverſis, claviculæ dextræ extrema portio, quæ ſcapulæ nectitur, ſiniſtræ media portio lapide tecta. Ex qua ſceleti proportione judico, integram hominis ſtaturam fuiſſe eandem, quæ incirca mea eſt, 58½. pollicum Pariſienſium.

Tiguri, 25 *Decemb.*
1725.

IV. *Obſer-*

IV. *A Contrivance to avoid the Irregularities in a Clock's Motion, occasion'd by the Action of Heat and Cold upon the Rod of the* Pendulum. *By Mr.* George Graham, *Watch-Maker, F. R. S.*

WHereas several, who have been curious in measuring of Time, have taken Notice, that the Vibrations of a Pendulum are slower in Summer than in Winter; and have very justly supposed this Alteration has proceeded from a Change of Length in the Pendulum itself, by the Influences of Heat and Cold upon it, in the different Seasons of the Year; with a View therefore of correcting, in some degree, this Defect of the Pendulum, I made several Trials, about the Year 1715. to discover whether there was any considerable Difference of Expansion between Brass, Steel, Iron, Copper, Silver, *&c.* when exposed to the same Degrees of Heat, as nearly as I could determine; conceiving it would not be very difficult, by making use of two Sorts of Metals, differing considerably in their Degrees of Expansion and Contraction, to remedy, in great measure, the Irregularities to which common Pendulums are subject. But altho' it is easily discoverable, that all these Metals suffer a sensible Alteration of their Dimensions by Heat and Cold; yet I found their Differences, in Quantity from one another, were so small, as gave me no Hopes of succeeding this Way, and made me leave off prosecuting this Affair any farther at that Time. In the Beginning of *December*, 1721. having Occasion for an exact Level, besides other Materials I made Trial of, Quicksilver was one; which, although I found it was by no Means proper for a Level, yet the extraordinary Degree

gree of Expanſion, that I obſerved in it, when placed near the Fire, beyond what I had conceived to be in ſo denſe a Fluid, immediately ſuggeſted to me the Uſe that might be made of it, by applying it to a Pendulum. In a few Days after, I made the Experiment, but with much too long a Column of Quickſilver, the Clock going ſlower with an increaſe of Cold, contrary to the common Pendulum ; however, it was a greater Confirmation of the Advantage to be expected from it, ſince it was eaſy to ſhorten the Column in any Degree required. The only Doubt I entertained, was, leſt there ſhould not be a proportional Expanſion and Contraction between the Quickſilver, and the Rod of the Pendulum, through the various Degrees of Heat and Cold, from the one Extreme to the other. To make this Experiment the more convincing, I placed the Clock in a Part of the Houſe, the moſt expoſed of any to the Changes of Heat and Cold, the Room having no Fire in it in the Winter, and expoſed to a South Sun, with Leads above it, which, in the Summer, made it extremely hot. I hung a Thermometer by it, and had likewiſe another Clock at no greater Diſtance from it, than was neceſſary to keep the Caſes from touching one another. This Clock had been made ſome Years before, with extraordinary Care, having a Pendulum about 60 Pounds in Weight, and not vibrating above one Degree and half from the Perpendicular ; and which, in a more temperate Situation, had not alter'd above 12 or 14″ in 24 Hours, between Winter and Summer ; but in this Place it altered 30″ a Day, between the hotteſt and coldeſt Weather, in the Year 1722, a Year no way remarkable for either Extremes. But this great Alteration was owing to the Situation I mentioned above, and which I made Choice of for the ſake of making the Experiment the more ſenſible.

The two Clocks being firmly skrewed to a Party-Wall, I began to make the firſt Trial of this Kind of Pendulum, *December* 18. 1721. and by *January* the third, perceiving the Pillar of Quickſilver conſiderably too long, I procured a ſhorter Glaſs, which I got ready by the eighth, and made uſe of, until the Beginning of *June* following: By which Time I was well ſatisfied of the Advantage of the Contrivance, notwithſtanding both theſe Pendulums were but rudely executed, and this laſt had the Pillar of Quickſilver too ſhort, but much nearer the true Length than the firſt. This encouraged me to provide another Glaſs, a little longer than the laſt, and to beſtow more Care upon all the Parts of the Pendulum that required Exactneſs. This being finiſhed, by the 9th of *June*, I began then to obſerve the Motion of the Clock, by the Tranſits of the fixed Stars, as often as the Weather permitted, making uſe of a Teleſcope which moved in the Plane of the Meridian; with this Inſtrument I could be ſure of not erring above two Seconds in Time. The Clock was kept conſtantly going, without having either the Hands or Pendulum alter'd, from the 9th of *June*, 1722. to the 14th of *October*, 1725. being three Years and four Months.

For the firſt Year, I wrote down every Day, the Difference between the two Clocks, with the Heighth of the Thermometer, not omitting the Tranſits of the Stars, as often as it was clear. The Reſult of all the Obſervations was this, That the Irregularity of the Clock, with the Quickſilver Pendulum, compared with the Tranſits of the Stars, exceeded not, when greateſt, a ſixth Part of that of the other Clock with the common Pendulum; but for the greateſt Part of the Year, not above an eighth or ninth Part; and even this Quantity would have been leſſened, had the Pillar of

Mercury been a little ſhorter; for it differed a little the contrary Way from the other Clock, going faſter with Heat, and ſlower with Cold; but I made no Alteration in the Length, to avoid an Interruption of the Obſervations. To confirm this Experiment the more, about the Beginning of *July*, 1723, I took off the heavy Pendulum from the other Clock, and made another with Quickſilver, but with this Difference, that inſtead of a Glaſs Tube, I made uſe of Braſs, and varniſhed the inſide, to ſecure it from being injured by the Mercury. This Pendulum I have made uſe of ever ſince, and find it about the ſame Degree of Exactneſs as the other. The Reaſon, why this kind of Pendulum is more exact than the common Sort, will be evident to any one, who conſiders, that as Heat lengthens the Rod of the Pendulum, at the ſame Time it increaſes the Length of the Pillar of Quickſilver, and its Center of Gravity is moved upwards: And when, by Cold, the Rod of the Pendulum is ſhortened, the Pillar of Quickſilver is likewiſe ſhortened, and its Center of Gravity carried downwards; by this Means, if the Column of Quickſilver be of a proper Length, the Diſtance, between the Point of Suſpenſion and the Center of Oſcillation of the Pendulum, will be always nearly the ſame, upon which the exact Motion of a Clock principally depends. Were the Pendulum of a Clock to remain invariably of the ſame Length, yet ſome little Inequalities would appear in its Motion, from the Difference of Friction ariſing from the Imperfections of the Materials, as well as different Degrees of Foulneſs; upon which Account, the Force communicated to the Pendulum, would not be conſtantly equal, which would cauſe ſome ſmall Alteration. But when the Pendulum is very heavy, and vibrates in a ſmall Arch, and the Workmanſhip of all the Parts is well performed

formed, there will be very little Inequality in the Motion, beſides what proceeds from Heat and Cold.

In making uſe of Quickſilver for a Pendulum, by varying the Diameter of the Veſſel that contains it, or the Thickneſs of the Rod of the Pendulum, whether it be of Braſs or Steel, they may be reduced nearly to an Equality as to the receiving, or retaining the Impreſſions of Heat or Cold, upon which the greater Regularity of the Motion depends; and particular Care ought to be uſed to free the Mercury from all Blebs of Air, otherwiſe their great and ſudden Expanſion, or Contraction, may cauſe a conſiderable Diſorder; but the Air may as eaſily be excluded in this Way, as in a Barometer, and the great ſpecifick Gravity of Quickſilver, renders it a proper Material for the Weight of a Pendulum.

FINIS.

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PHILOSOPHICAL TRANSACTIONS,

For the Months of *March* and *April*, 1726.

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V. *A*

V. *A New and Exact Table, Collected from ſeveral Obſervations, taken in four Voyages to* Hudſon's-Bay *in North* America, *from* London: *Shewing the Variation of the Magnetical Needle, or Sea-Compaſs, in the Path-way to the ſaid* Bay, *according to the ſeveral* Latitudes *and* Longitudes *from the Year* 1721, *to* 1725. *By Mr.* Chriſtopher Middleton.

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ERRATA.

PAGE 51. line 8. *lege & 1 parte tartari.* p. 69. l. 1. *dele* III. p. 71. l. 2. *dele* IV.

I. *De*

I. De Salibus Alcalino-fixis, *Auctore* C. Neuman *Chym. Prof. Reg.* Berolin. *R. S. S.* **Pars reliqua.*

VI. *An Salia Alcalino-fixa inter ſe differant, & qua in re?*

EQuidem de hoc momento jam hinc inde quædam inſerui; nihilominus neceſſarium eſſe exiſtimo, diverſa quædam momenta, præter ea quæ curſim adduxi, hac de re adhuc in medium proferre.

De Salibus lixivioſis plantarum primum *in genere notandum* venit, Quod in quantum forſan una vel altera differentia interea ſeſe exſerit, iſta *plane non à ſpecifica indole & ſingularibus viribus*, quas una vel altera planta *ante combuſtionem & incinerationem poſſedit*, ortum trahat, ſed differentia iſta primario dependeat *à duabus aliis circumſtantiis*; (prout hoc experientiſſimus & inſignis Chymicus D. D. *Stahl* jam animadvertit) nimirum 1. *à diverſa ſtructura* vel teneriorum vel craſſiorum partium plantæ; & 2. *ab operatione*, vel mutatis in ea enchireſibus & parvis manipulationibus.

1. *Ratione ſtructuræ* acquiritur e. g. longe aliud Sal Alkali ex ſubtilibus & teneris foliis, quam ex craſſioribus caulibus ejuſdem herbæ; aliud e petalis florum, vel totis floribus, quam e lignis; aliud ex ſeminibus, quam ex radicibus; aliud ex ſucco expreſſo, quam ex extracto, quod ex ſiccata herba cum multa aqua & forti coctione paratum erat; aliud ex teneris recentibus herbis, quam ex valde exſiccatis; & ſic porro.

* V. *Phil. Tranſ.* N°. 392.

2. *Ratione operationis*, acquiritur longe aliud Sal Alkali è leni tantum fumante excandeſcentia & incineratione, quam è forti flammante combuſtione; aliud ab elixiviatione cum aqua frigida, quam cum aqua calida, imo cum reali excoctione ebullitoria; aliud ſi parum aquæ aſſumas, quam ſi multum aquæ adhibeas; aliud ſi parum & ſemel tantum calcines, quam ſi fortiter & ſæpius calcinatio reiteretur; aliud ſi Sal inſpiſſatum cum multa aqua iterum ſolvas, quam ſi per ſe in deliquium fluere permittas; idq; rurſus ſecundùm plures etiam alias circumſtantias.

Sed, *niſi* Salia lixivioſa de *induſtria, forte per repetitas calcinationes, vel deliqueſcentias*, ad nimiam & maxime notabilem differentiam redigantur, in communibus ordinario uſitatis laboribus & præparationibus *horum ſalium* reperibilis *differentia parva vel nullius eſt conſiderationis*, excepto *ſi forte minerale* quippiam, *vel de induſtria, vel incuria, vel inſcitia adhibitum fuit.*

Quod ſi ergo apud ordinaria pure præparata ſalia lixivioſa herbarum, diverſitas quædam tamen reperiretur, in hoc tantummodo conſiſteret, quod unum magis pure alcalinum quam aliud, unum ſubtilius, aliud craſſius, unum magis terreſtre quam aliud, unum paulo magis cauſticum quam aliud, unum cum pluribus ab oleoſo ingrediente oriundis partibus, vel etiam copioſiore ſpiritu acido imbutum, indeque magis cryſtallinum ſit, quam alia ſalia: de cetero *ſingula ſunt ſalia lixivioſa, ſingula ſalia fixa & alcalina, ſed ſingula non ſunt vere pura, cum puriſſima ex iis adhuc ſint impura, & ad alias chymicas applicationes adhuc inepta ſalia alcalina*, dum pleraque apertum quippiam & ſuperfluum acidum, quædam etiam ſuperfluam terram aliaſque accidentales peregrinas immixtiones, ſi non voluntarias ineptas additiones, tanquam putatitia artificia pro ac-

quirendis ſpecioſis cryſtallis, ſecum foveat: Ita ut, ſi *ordinaria in Pharmacopoliis parata vegetabilium cryſtallina lixivioſa Salia*, animo revolvo, ſæpe non ſatis mirari poſſim, quare iſtiuſmodi ſalia in hodierna praxi medica adhuc toto die adhibeantur, cum tamen medico ignotum ſit, quale ſal ægrotis commendet, aut præſcribat, *quia ejuſmodi cryſtalliſata ſalia nec pura alcalina, nec perfectè ſaturata ſalia neutra exiſtunt, & per conſequens accurate novit nemo, quantum alcali, & quantum acidi una drachma vel uncia talis pulchre cryſtallini ſalis contineat*, item, *an præſens ibi & ad cryſtalliſationem diſponens acidum, vegetabile, vel minerale acidum ſit.*

Quod ſi egomet in praxi medica *Sale Alcalino fixo* opus haberem, tum purum *ſal tartari* vel etiam depuratos *cineres clavellatos* reciperem; quod *ſi ſal acidum* ex uſu foret, tum ex apertis *puris acidis* commodiſſimum ſeligerem, & *ſi ſal medium* adhibendum eſſet, itidem ejuſmodi *ex uſitatis plene ſaturatis ſalibus mediis*, quæ me certiorem reddunt, & quod ad præſentem ægritudinem pro commodiſſimo reputarem, in uſum vocarem; tum enim certo ſcirem, quid ex pharmacopolio daretur, & quid ægroti ſumerent, reliqua autem *incerta* & imperfecte ſaturata *ſalia, quæ nec pura ſalia acida, nec alcalina, nec media ſeu neutra ſunt, ſecure negligerem* & ſuo loco relinquerem.

Iſtam adhuc admonitionem & conſilium dare volo, quod ſi forte adhuc unum vel alterum ſingularem effectum ab hoc vel illo ſale vegetabili lixivioſo expectare velis, & quod ſi poſſibile eſt, ut quoddam Sal Alcalino-fixum adhuc ſingulare quippiam in re medica præ alio poſſidere queat, ut ejuſmodi herbam ad minimum quam *leniſſime excandeſcere* ſinas, *non violenter incineres, multo minus fortiter exuras*, omnium minime vero *nec cineres nec ſal calcines*, tum forte

ſpecifica quæ expectatur virtus, *in paucis* quibuſdam reſtantibus revera *oleoſis partibus* (quippe in quibus tantummodo ſitam eam eſſe oportet) aliquo modo conſervaretur; cum contra certior ſim, quod *ſi* abſque conſideratione & cautione *igne flammæ exuratur, calcinetur & depuretur*, quod inquam revera *nihil ſpecifici* nec ſingularis amplius *reperiri aut demonſtrari* queat.

Quanquam igitur curioſitatis gratia, *exuna eademque planta abſque ullo peregrino adâitamento*, unice *mediante igne & aqua*, ut non dicam 40 vel 50, dicam tantummodo permulta genera ſalium alcalinorum fixorum, quorum aliud ab alio ſemper aliqua in parte, licet parum tamen aliqua in re, modo externo aſpectu, modo etiam interna leviore circumſtantia differre debet, præparare & producere norim; tamen altera quoque ex parte non ignotum mihi eſt, quod non tantum hæc multiplicia ex una eademque planta artificioſe producta, ſed & *omnia lixivioſa herbarum ſalia*, imo quod plus eſt, *omnia ſalia alcalino-fixa in orbe reperibilia ad eundem aſpectum, virtutem & efficaciam reduci poſſint:* ita ut hoc reſpectu & intellectu non niſi unicum & unius generis ſal Alcali fixum ex omnibus vegetabilibus in orbe ſtatuam, dum vel hoc unicum in varias formas & effectus, vel etiam omnia alia ad eundem aſpectum & effectum reducere poſſum, adeo ut *ſi ſalia lixivioſa ex centum vel pluribus herbis parata* ante oculos haberem, *ullum ſal, multo minus ullum ſubjectum* ex quo ejuſmodi ſal Alkali productum fuerit, *divinare poſſit nemo.* Quin nemo potis eſt tantummodo in genere cognoſcere, num ex eſculento aut innocuo, vel ex ſumme venenoſo & maxime nocivo vegetabili confectum ſit; an ſubjectum ſuaveolens, vel graveolens, plane abſque ullo odore vel fœtore fuerit; an ſapidum vel inſipidum;

purgans vel adſtringens; antiſcorbuticum, epilepticum, vel antifebrile, emeticum, vel diureticum, narcoticum, vel ſternutatorium, cauſticum, vel balſamicum; an ligna, vel radices, cortex, vel medulla, flores, vel herbæ, ſemina, vel ſucci, vel quicquid fuerint, ex quibus ejuſmodi præſens ſal alcalinum paratum ſit.

Exiſtimo hæc ſufficere poſſe pro iis, qui adhuc conſtanter opinantur, *Salia Alcalina fixa ſpecificam* herbarum aliorumque vegetabilium, ex quibus producuntur, *virtutem ſecum retinere*, ita ut ſal caryophyllorum & cinnamomi ſtomachicum, ſal hyoſciami & papaveris narcoticum, ſal radicum jalappæ purgans, ſal radicum tormentillæ, vel corticum chinæ chinæ adſtringens vel antifebrile, ſal ipecacuanhæ & gratiolæ emeticum, ſal eſulæ cauſticum & ſal cicutæ venenoſum & lethiferum eſſet, convincendis, quin & univerſos reſurrectionis fratres ita confundendo, ut reſuſcitationem & revivificationem vegetabilium non ita facile concipere debeant, dum demonſtrari ipſis poteſt, omnia ejuſmodi ſalia lixivioſa in toto Regno Vegetabili ad eandem figuram & effectum, imo (ut ante dixi) ex eadem herba ad tam multiplicia ſalia reduci poſſe: unde aperte hic profiteor, *nemini hominum poſſibile eſſe, ex cineribus, vel lixivio ne quidem ſolam externam figuram iſtius vegetabilis, ex quo parata ſunt, rurſus producere, vel in ſale ipſo ex ejuſmodi puris ſalibus repræſentare*; de induſtria dico *ex ejuſmodi puris ſalibus*; ſiquidem ipſe non una vice vidi, ejuſmodi figuras herbarum *ex teneris æneis, & ſubtilibus linteis filis* præter propter *artificioſe concinnatis*, & in lixiviis concentratis impoſitas vel ſuſpenſas fuiſſe, ubi utique omnia ad ejuſmodi fila circumcirca cryſtalliſata fuere, quæ deceptor ignaris *tanquam veram reſuſcitationis plantarum repræſentationem* commonſtravit: verbo, tota quæ prætenditur *humana*

arte

arte reſuſcitatio, in veritate non fundata, ſed tantum chimerica imaginatio eſt, & non niſi in libris viſibilis & reperibilis, ita ut de hac materia jure dici poſſit:

Dico ego, tu dicis, ſed denique dixit & ille,
Dictaque poſt toties nil niſi dicta vides.

Reperitur etiam *inter puriora & ad chymicos labores aptiora cognita fixa alcalica ſalia* ipſa, in hac vel illa parte *quædam differentia. quæ vero* itidem ab exercitato chymico *omnia removeri,* ea penitus *æqualia & ad eandem figuram & effectum reduci poſſunt.*

D. D. *Hoffmannus* Profeſſor *Halenſis,* amicus meus æſtimatiſſimus in Libro 2. obſervationum Phyſ. Chym. ſelectiorum ſub N°. 29. totum caput hac de re conſignavit, *nihil tamen quam differentias ipſas obſervavit & protulit,* quæ præter propter & ſummatim in his conſiſtunt.

1. Si oleum vitrioli nitro fixo affunditur, tum ſub efferveſcentia verus ſpiritus nitri, vel aqua fortis exhalat.

2. Hoc vero nunquam fit, ſi oleum vitrioli ſali tartari, vel cineribus clavellatis affundatur.

3. Quin & in his utriſque poſterioribus rurſus quædam obſervatur differentia, dum a ſale tartari (ex affuſione olei vitrioli) ſuperficies nigricantem cuticulam acquirit, liquor ipſe nigricans evadit, & plane alius fœtor, quam ſpiritus nitroſi, à nitro fixo deprehenditur.

4. Omnia autem hæc a cineribus clavellatis non obſervantur, dum ab iis nec ſpiritus nitri, nec cuticula, nec nigricans color ſeu ſolutio, ſed iterum ſingularis vapor apparet.

5. Fumi

5. Fumi hi nitroſi etiam manifeſti fiunt, ſi oleum vitrioli ſali tartari extemporaneo, quod ex 2 partibus nitri & una parte tartari paratur, affunditur.

6. Quod vero iterum non licet videre in ejuſmodi ſale tartari extemporaneo, quod cum pauca additione nitri elaboratum fuit.

7. Ex ſale tartari & arena vitrum clarum & pellucidum parari non poteſt.

8. Bene autem ex ſale tartari extemporaneo, ut & ex cineribus clavellatis.

9. Ex ſale tartari, vel cineribus clavellatis in crucibulo fuſis, adjectis carbonibus, reſultat ſpecies hepatis ſulphuris, quale tamen nec nitrum fixum, nec nitrum cauſticum conſtituit.

10. Sal tartari & nitrum cauſticum cum ſpiritu vini rectificatiſſimo tincturam largiuntur, quod vero nec nitrum fixum, nec cineres clavellati, nec iſtud ſal tartari extemporaneum, quod ex 2 partibus nitri & 1 parte tartari paratur, depromunt.

Has quidem *differentias* laudatus D. D. *Hoffmannus advertit, ſed non adduxit, unde hæc eveniant, qua in re differentia hæc ſita ſit, an, & quomodo iſta meliorari & corrigi queant, multo minus quomodo hæc adeo differentia ab ipſo reperta ſalia alcalino-fixa penituſque æqualia reddi poſſint*, cum tamen omnia & ſingula naturali ordine procedant, differentia non adeo difficulter percipi & facile corrigi, & unum ſal alteri æquiparari poſſit, adeo ut *primum* ne unicum quidem phænomenon, ex omnibus ſupra allegatis, uni vel

alteri

alteri ſali ſingulare exiſtat, & *ſecundo* inter omnia pura fixa alcalica ſalia nullum aliud diſcrimen amplius, niſi circa gradus cauſticitatis adverti debeat.

(*a*) Affuſum oleum vitrioli in nullo dictorum ſalium ſingularem, ſed ſemper eundem vaporem & fœtorem producere debet.

(*b*) Omnia hæc ſalia æqualiter in clarum & pellucidum vitrum redigi debent.

(*c*) Singula etiam ſpiritum vini rectificatiſſimum pulchro colore tingere debent, & ſic porro.

Sic non neceſſum eſt mirari, *quod oleum vitrioli ex nitro fixo*, vel *ex ſale tartari extemporaneo*, *quod cum 2 partibus nitri* paratum fuit, *ſpiritum nitroſum ejiciat*, dum, ſi hoc apparet, in ejuſmodi ſale Alkali neceſſario *adhuc* quædam *nitri crudi portio* ineſt, unde exundantes ex eo fumi (qui nihil aliud quam verum ſpiritum nitri conſtituunt) adeo naturales ſunt, ac ſi Glauberiana methodo oleum vitrioli nitro crudo affunditur, cum e contra utique ex *ſale tartari vulgari*, *ex cineribus clavellatis*, *aliiſque* ejus generis *alcalinis ſalibus*, *quibus nitrum non accedit*, imo *N. B.* quibus non ſuperflue accedit, etiam *nullus ſpiritus nitri* liberari, vel fumus nitroſus exſpectari poteſt.

Hæc res quo tanto certius pateat, recipiatur tantum *idem nitrum fixum*, *vel idem ſal tartari extemporaneum*, à quo ab affuſione olei vitrioli fumus nitroſus apparet, idque denuo cum uno vel alio ſubjecto inflammabili Regni vegetabilis commiſceatur, *e. g.* nitrum fixum *cum majori* carbonum *quantitate*, & ſal tartari extemporaneum *cum copioſiore tartaro* conjungatur, & deflagret ut ante, vel majoris certitudinis cauſa

cauſa proportio carbonum, vel tartari adhuc plus augeatur, quo ſuperfluum antea, & adhuc crudum exiſtens nitrum plus inflammabilis terræ reperiat, quacum incendatur & alcaliſetur, *ut* per conſequens *nihil nitri crudi amplius reſtare poſſit*, tum revera ex hoc recollecto ſale, ab affuſione olei vitrioli, *nullus* ſpiritus nitroſus amplius, ſed potius plane alii vapores effumabunt.

Quod *oleum vitrioli cum ſale tartari* fœtorem ſpiritui nitri plane diſſimilem, nigricantem cuticulam, & obſcuram ſolutionem efficiat, hoc a nulla alia re, niſi *a* ſuperfluis adhuc *in ſale tartari reſiduis empyreumaticis* partibus unice dependet; quod ſi ſal tartari ab iſtis partibus decenti modo repurgatum eſt, tum itidem nec fœtor amplius, nec nigricans ſubſtantia, utpote *quæ oleum vitrioli cum omnibus oleoſis contrahit*, apparebunt: unde ex *cineribus clavellatis*, quia iſti, mediante calcinatione, *ab oleo empyreumatico* magis *liberati* ſunt, ut & ex *puro ſale tartari*, ſi ſufficienter calcinatum eſt, ejuſmodi nigredo nunquam apparet, nec demonſtrari poterit, ſed ab affuſione olei vitrioli omnia clara, & albida permanent.

Simili ratione *hoc oleum empyreumaticum in ſale tartari* unica exiſtit cauſa, quare cum arena clarum pellucidum vitrum non largiatur; ſimul ac ſuperflua hæc oleoſa ſubſtantia ipſi dempta fuit, tum eodem modo, adeo aptum & commodum eſt ad pellucidum vitrum conficiendum, ac *cineres clavellati*, *vel ſal tartari extemporaneum* ipſum: hanc rem declarat & confirmat, quod ſub ejus præparatione, durante detonatione cum nitro, *omnis oleoſa tartari ſubſtantia combuſta & deſtructa* eſt: unde hoc Sal Alkali, quamvis tartarus ad ejus præparationem acceſſerit, per conſequens eodem modo vitrum clarum efficit, quam cineres clavellati, ad quos nihil de tartaro aſſumitur.

Quinetiam hoc in tartaro adhuc præſens, vel alio quoque modo ſali alcalino conciliatum *Phlogiſton*, utpote quod tantum & unice [prout jam ſæpius monui] *ad cauſticitatem* confert, eſt ſubſtantialis iſta materia, quæ ſpiritui inflammabili rectificatiſſimo *tincturam* conciliat, quanquam etiam ex parte ſpiritus ardens, qua menſtruum, ſi nempe ille *olei dives* ſit, non parum ad hoc contribuat, & *tinctura tanto magis acceleretur, & augeatur.* Quid mirum igitur, ſi ex ſale tartari extemporaneo cum 2 & 3 partibus nitri parato (quippe per quod omne inflammabile, & ad tincturam producendum abſolute neceſſarium deſtruitur) nullam tincturam recipias?

VII. *De relatione Salium Alcalino-fixorum, ad alia mixta chymica, eorumque uſitata hactenus applicatione ad Chymiam.*

QUanquam ſub definitione horum ſalium dixerim, ea conſiſtere ex terra, in qua parum fixioris vel ultimo magis concentrati ſalis acidi, & inflammabilis, mediante igne, toſtione quaſi & aſſatione impactum eſt, attamen eodem tempore ſimul adduxi, ut ex ipſis definitionis verbis, PARUM VEL ALIQUID, apparet, ſuperfluum ſive acidum, ſive inflammabile, imo ipſas terreſtres ſuperfluas partes, non modo non neceſſaria, ſed, & in reſpectu puritatis & perfectionis, quas ejuſmodi ſal requirit, præjudicioſa & nociva eſſe.

Quod ſi purum quoddam Sal Alcali-fixum, ſive de induſtria, ſive per accidens, cum ſatis notabili & aucta portione unius antedictarum conſtituentium partium, ſive ſit nimium acidi, ſive nimium inflamma-

Philo Tran: 418.

Tab. I.

Fig I.

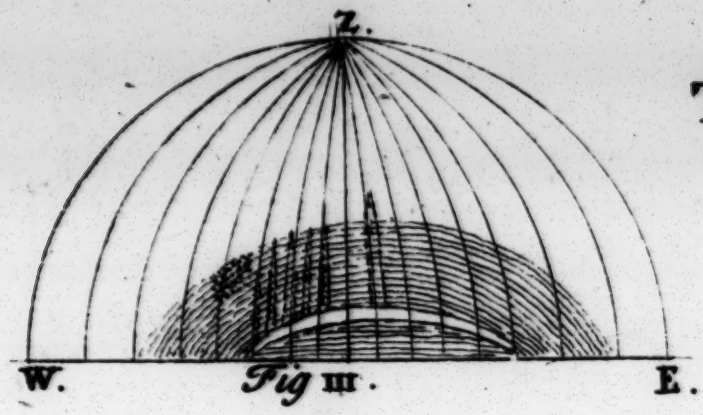


Fig II.

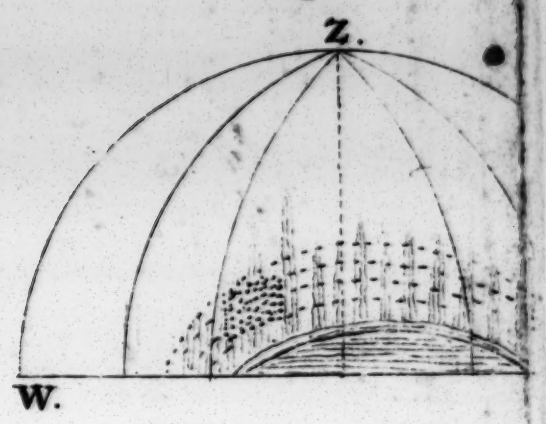


Fig III.

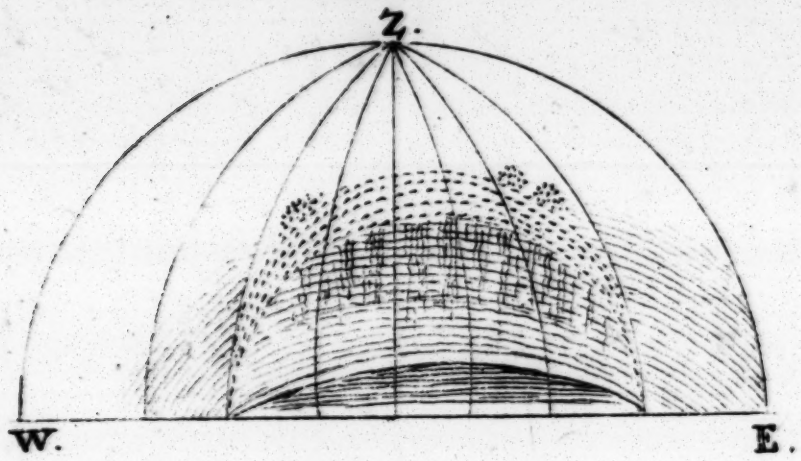


Fig IV.

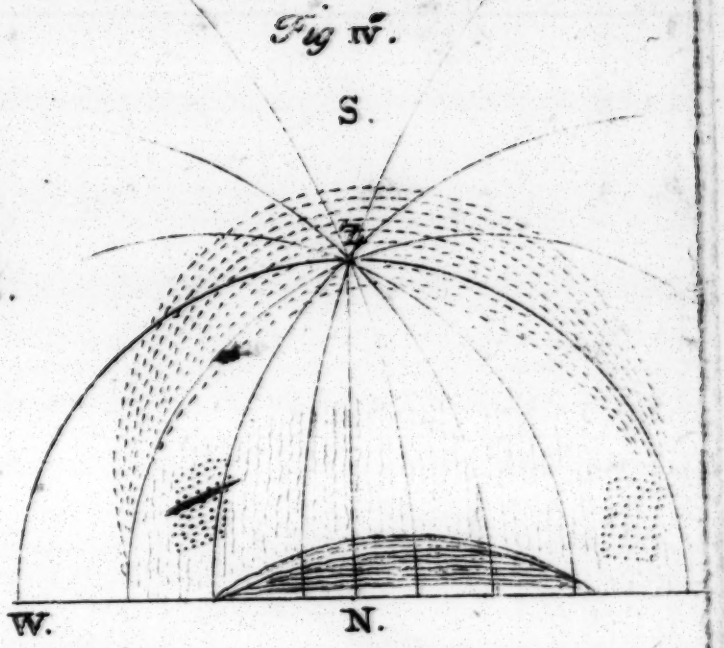


Fig V.

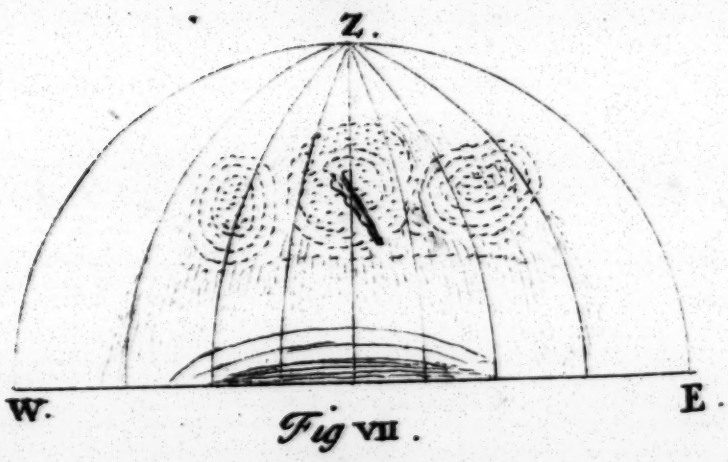


Fig VI.

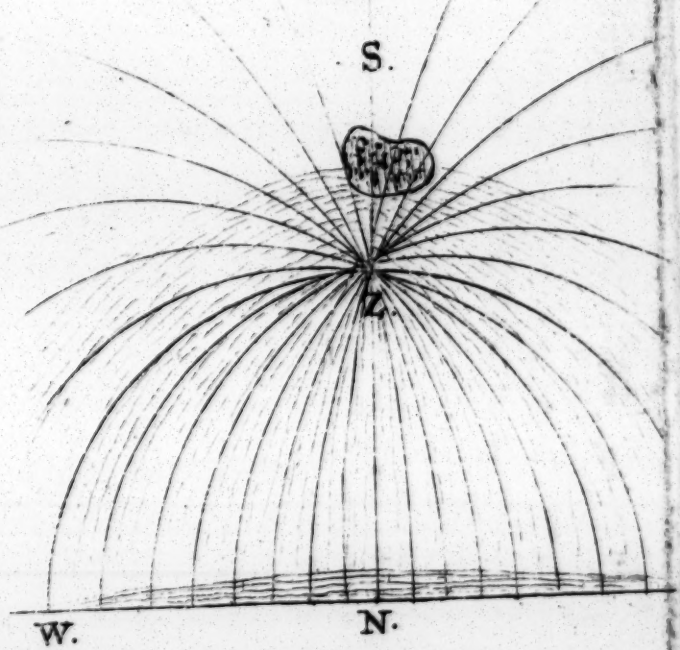


Fig VII.

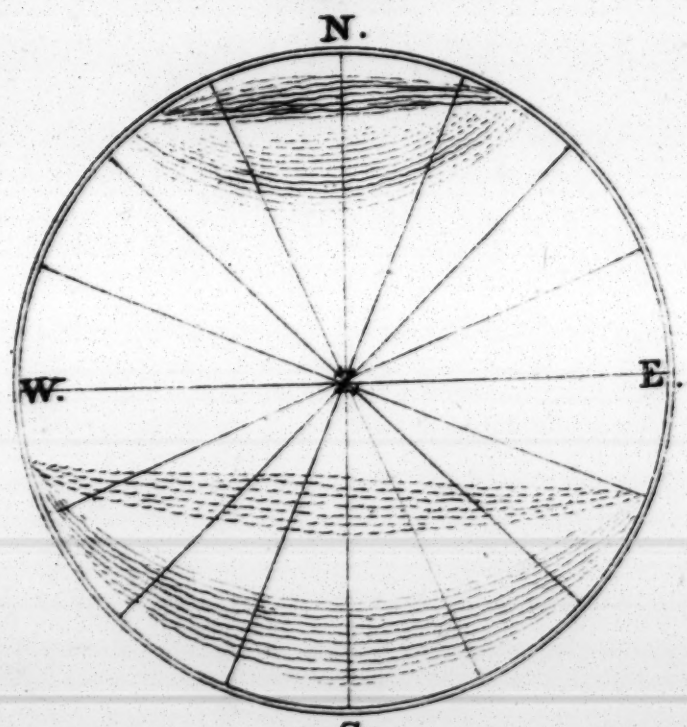
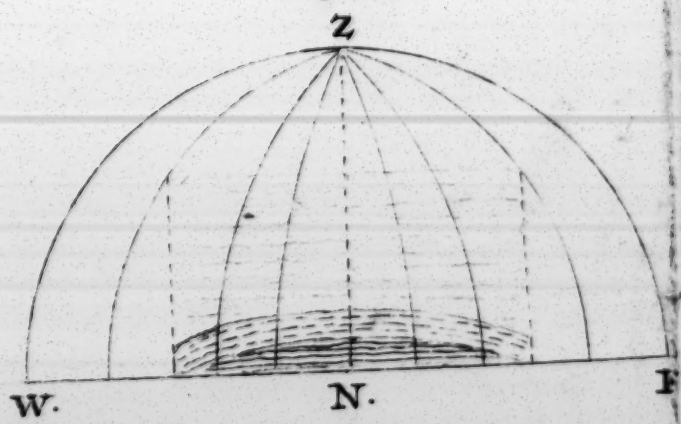


Fig VIII.



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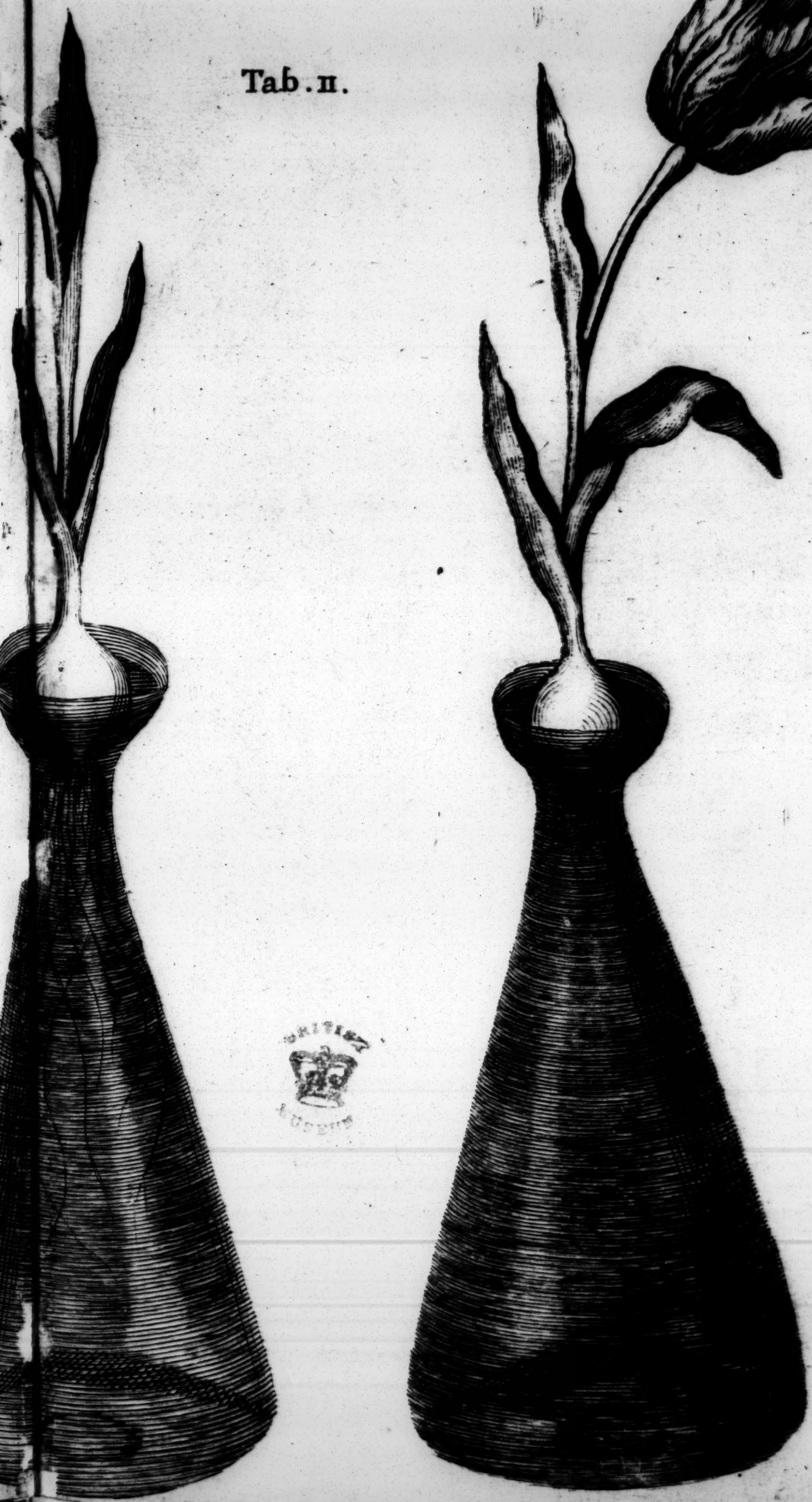
P55 VI

Phil: Tran: N.° 418.

Tab. II.



E.

bilis, ſive nimium partium terreſtrium, vel cum duobus horum ſimul, denuo tractatur & commiſcetur; tum novum hoc commixtum rurſus, *non modo aliam* plane *denominationem*, ſed & *plane alias qualitates & effectus*, ac antea ſolum Sal Alcali habuit, ſortitur.

Nam, ſi Sal Alcali-fixum cum ſale acido tam diu commiſcetur, donec, poſt remiſſionem efferveſcentiæ, & ſeparationem aquæ, utpote quæ tantum pro vehiculo ſolutionis inſerviit, ſyrupum violarum, ſi ejus aliquid admiſcetur, in ſuo colore non alteret, eumque nec rubrum, nec viridem reddat, tum appellatur *nova hæc mixtio ſal medium*, *ſal ſalſum*, vel *ſal neutrum*. Quod ſi acidum minerale ad hoc adhibitum fuit, tum novum ejuſmodi mixtum facile *in cryſtallos* concreſcit; ſi vero acidum vegetabile uſurpatum fuit, tum *difficulter*, *vel plane non cryſtalliſari* poteſt.

Cum autem in chymia *valde diſtincta ſalia acida* occurrant, proinde etiam juxta indolem iſtorum acidorum [ſi unum, vel alterum eorum, cum Sale Alcalino-fixo uſque ad ſaturationem unitum eſt] exinde productum *ſal medium* ordinario *denominationem accipit*, e. g.

1. Si Sal Alcali-fixum *cum acido vitriolico*, *ſeu ſulphureo*, ita commiſcetur, ut *cryſtalli* exinde emergant, tum ex his utriſque obortum cryſtallinum ſal medium plerumque *tartarus vitriolatus*, vel *ſal polychreſtum* vocatur.

2. Si Sal Alcali-fixum, *cum acido nitri* commiſcetur, & cryſtalliſatur, perfectum inde *nitrum* cryſtallinum exoritur, quod *nitrum regeneratum*, vel etiam *tartarum nitratum* appellare conſueverunt.

3. Si *cum acido ſalis communis* ſaturatur, fit inde *ſal commune regeneratum*, ita dictum *ſal embryonatum*, vel *digeſtivum* Sylvii.

4. *Cum acido aceti* fit *terra foliata tartari*, ſeu, ſic dictum *arcanum tartari.*

5. *Cum acido tartari* oritur *tartarus ſolubilis*, ſeu *tartarus tartariſatus.*

6. *Cum ſucco citri tartarus citratus.*

7. *Pars alcalina*, quæ *in ſale communi* reſidet, *cum acido vitriolico* ejuſmodi ſal medium conſtituit, quod *ſal mirabile* Glauberi vocant.

8. Quod ſi *pars alcalina nitri cum* dicto *acido vitriolico* commiſcetur, ita ut ex his duobus ingredientibus *cryſtallinum ſal medium* paretur, illud, quanquam in fundamento à tartaro vitriolato nihil differat, aliud tamen nomen recipit, dum ſcilicet ab innumeris annis *arcanum duplicatum*, vel *panacea Holſatica* appellatur.

9. Quod ſi vero ordinariis alcalinis plantarum ſalibus tantum ſuperfluum quicquam acidi, ſive à parte vegetabilis ipſius, ſive aliunde accedat, tum communiter inde *ſal cryſtallinum plantarum lixivioſum* oritur.

Si *Sal Alkali-fixum* cum *inflammabili* quodam miſceatur ſive ſint pinguedines, ſive expreſſa, quin & certis ſub enchireſibus deſtillata olea, exſurgit inde pro primo [uti notum eſt] SAPO, deinde vero etiam *ſal*

volatile & ſpiritus urinoſus; quod ſi vero olei empyreumatici animalis quippiam accedit, tum hoc, cum eo etiam tantum imprægnatum, Sal Alcali, præ inflammabili vegetabilium, in curioſis quibuſdam laboribus non parum ſeſe diſtinguet.

Si bituminoſum & acidum vitriolicum mixta [nempe *in ſulphure* communi] cum Sale Alcalino-fixo concurrunt, gignitur inde *hepar ſulphuris*.

Denique, ſi purum Sal Alkali-fixum in igne, cum terra ſaturatur, puta pellucida, ut *arena* communi, *ſilice*, *pyrite*, *glarea*, & ſimilibus, tum non tantum *ſubtilis ſpiritus acidus* inde extorquetur, ſed & totum mixtum convertitur in pellucidam & tranſparentem, in igne quidem adhuc fuſibilem, in aqua vero amplius *indiſſolubilem terram*, nempe *vitrum*, vel etiam cum aliis additamentis in *porcellanam*.

In genere Salia Alcalia-fixa, non facile *ſeſe uniunt*, vel hæc ſalia non facile quicquam *diſſolvunt*, quod non vel *inflammabilitatem*, vel aciditatem quandam continet, & *unum ſemper lubentius & intimius quam alterum*.

1. Sic Salia Alcalia-fixa facile quidem *diſſolvunt ſulphur* commune, quippe in quo *inflammabile & acidum* unita occurrunt, verum *facile* quoque ab eo rurſus *ſeparantur* & liberantur.

2. Facile quoque diſſolvunt, & ſeſe uniunt cum *axungiis*, *pinguedinibus* & *oleis expreſſis*, & communicant pinguibus hiſce materiis *diſpoſitionem* ſeſe cum ipſis prompte *in aqua ſolvendi*, & cum aqua ſeſe uniendi, cum alias iſtæ abſque Alcali-fixo unionem cum aqua omni modo fugiant & detrectent.

3. Salia fixa ſolvunt, & uniunt ſe quoque tandem cum *oleis deſtillatis* eſſentialibus, quamvis *paulo difficilius*.

Et

Et eadem cum differentia horum jam memoratorum duorum generum oleorum, eorumque faciliori vel difficiliori combinatione cum Alcali, *ſeparatio* quoque eorundem ſuccedit, dum Salia hæc fixa a pinguedinibus, axungiis & oleis expreſſis perquam *facile*, & ab oleis æthereis, vel eſſentialibus, contra perquam *difficulter*, *vel plane non* ſeparari queunt, præcipue ſi ſemel intime cum iis unita, & utraque *in perfectum ſaponem* redacta fuere.

4. *Separatio ex vitris* equidem *tarde* aliquatenus ſuccedit, interim poſſibilis tamen eſt factu, quamvis in uno citius quam in alio (præ reliquis in *vitro ſilicum* omnium *facillime*) dummodo denuo cum Alcali ſecundum artem miſceantur, & debito modo tractentur.

5. *Cum ſalibus acidis* hæc Salium Alcalino-fixorum *unio & ſolutio* fere omnium *facillime* & *promptiſſime* ſuccedit, & quidem *triplici modo*;

(*a*) Vel *miſcendo*, ſi utraque ſimpliciter abſque intermedio conjungantur;

(*b*) Vel *præcipitando*, ſi vel (α) ſal acidum, vel (β) ſal alcalinum fixum antea cum alio quodam corpore commixta fuere;

(*c*) Vel etiam *abſque præcipitatione*, dum ſal acidum equidem itidem antea cum alio quodam commixtum fuit, ſed hoc in acido ſolutum per præcipitationem exinde non ſeparatur.

1. *Miſcendo* peragitur hæc unio, ſi Sal Alkali fixum in aqua ſolutum, vel etiam quod per ſe in deliquium abiit,

abiit, cum ſpiritu, vel liquore quodam acido confunditur, prout in elaboratione diverſorum ſupra adductorum ſalium mediorum videre licet, *e. g.* in præparatione terræ foliatæ tartari, tartari tartariſati, nitri regenerati, tartari - vitriolati ex ſpiritu vitrioli & oleo tartari per deliquium, *&c.*

2. *Præcipitando* abſolvitur hæc unio (α); ſi *ſal acidum antea cum quodam* (A) *metallico*, vel etiam (B) *animali* combinatum & ſolutum fuit, id quod multiplici ratione, vel per accidens, vel de induſtria, fieri poteſt, & in Chymia Pharmaceutica ſæpius occurrit. (A) *e. g.* in præparatione auri fulminantis, in qua, ſi Sal Alcali ſolutum in aquam regiam auro ſoluto impræg-natam, infunditur, unio Salis Alcalici cum utriſque acidis (ſcilicet nitri & ſalis) præcipitando, vel cum proſtratione auri, contingit. Porro apparet hoc in præparatione mercurij præcipitati albi, in elaboratione tartari vitriolati Tachenii, & aliis metallicis præcipitationibus permultis. (B) circa *animales* ſolutiones videre licet ejuſmodi unionem Salis Alcalici cum acido itidem præcipitando factam, *e. g.* in præparatione magiſterii cornu cervi, aliarumque partuim animalium. (β) Alia ex parte peragitur hæc unio modo præcipitationis, *e. g.* in præparatione ſulphuris antimonii ſic dicti aurati, in qua *Sal Alcali primum* ſolutis ſulphureis partibus imprægnatum deprehenditur, quod tam cum affuſo ſale acido ſeſe unit, ſolutum vero antea ſulphur præcipitando deſerit; diſpaleſcit hoc porro, ſi commune ſmegma in aqua ſolutum cum acido præcipitatur, & hoc fit ubique, ubi craſſe pinguia reſinoſa, vel ſulphurea, cum Sale alcalino fixo antea ſoluta & combinata fuere, poſtea vero cum acido præcipitantur.

3. Tertia methodus unionis, quæ *abſque præcipitatione*, nec tamen ſimpliciter miſcendo abſolvitur, dum adhuc intermedium quoddam ſimul adeſt, *duplici modo* fieri poteſt; α. Vel

α. Vel *ſi ſal acidum antea cum ſale volatili urinoſo combinatum, & in ſal ammoniacale redactum fuit*, ubi, ſi huic ſali volatili medio Sal Alkali fixum adjungitur, ſal acidum, præcipue ſi lenis tantummodo calor applicetur, repente novo huic advenienti alcali fixo ſeſe aſſociat, & deſerit ſal alcali volatile, quod antea continebat, quod tum dicto leni calore, tanquam deſertum & in ſe perquam volatile corpus, facile expelli poteſt ;

β. Vel ſiſtitur etiam hic unionis modus, *ſi Sal Alcali antea cum debiliori acido ipſo combinatum fuit*, & fortius acidum affunditur, hoc novum mixtum in retorto vaſe igni exponitur : tum enim debilius acidum cogitur abſolute fortiori cedere, & deſtillando ſeparatur, quod in deſtillatione ſpiritus nitri & ſalis fumantis, aliorumque luculenter patet.

Quanquam autem hæc Salis Alcali fixi cum omnibus ſalibus acidis combinatio perquam *amice quaſi* peragitur, inde tamen *non exiſtimandum, ac ſi cum omnibus ÆQUALITER contingat*, vel cum uno eodem modo ac cum altero, *æquali firmitudine* ſeſe uniat. Minime ! ſed & hac in re *ingens occurrit differentia*, dum in uno vel altero præferentia (ut barbare loquar) ſeu *propinquior affinitas*, vel prout cuilibet denominare placet, *inter Sal Alkali fixum* & hoc vel illud *ſal acidum* obſervatur. Et cum hæc *affinitatis ſcala* in Chymia *non parvi emolumenti*, ejuſque notitia Chymico practico perquam neceſſaria ſit, proinde de ea, iis præcipue, quibus iſta adhuc forte ignota eſt, quædam in medium proferre ſuperſedere non poſſum, præprimis cum ex combinatione Salis Alcalino-fixi cum ſalibus acidis, & ex ſcala iſta, quam jam producturus ſum,

ſum, major *vis & fortitudo unius acidi præ altero*, omnium optime annotari & dijudicari poſſit, nempe *in quantum ad combinationem cum alkali fixo ſeſe refert.*

Purum ergo *Sal Alcali fixum* omnium optime & *firmiſſime* ſeſe combinat cum fortiſſimo præ omnibus acido primigenio, puro puto, & *fixiori acido vitriolico & ſulphureo*: quod ſi hoc acidum adhuc cum principio inflammabili, ut communi ſulphure, commixtum prodit, tum intima ejusmodi unio cum alcali non celebratur, ſed ſuperficialis tantum ſolutio ſeu cohæſio emergit, &, ut ſupra jam notavi, fit *nudum hepar ſulphuris*, ex quo alcali *facillime*, imo *cum debiliſſimo acido vegetabili rurſus ſeparari poteſt.*

Bonaex parte *firmius* acidum hoc ſulphuris Sali Alcalino cohæret, ut ad minimum a debiliſſimo acido liberari non poſſit, ſi *portionem* tantum *inflammabilis*, quocum conjunctum fuerat, per deflagrationem *amiſit*, quod clare ſatis patet, ſi durante deflagratione ſulphuris, Sal Alcalinum fixum ea ratione applicatur, ut volatilis ſpiritus ſulphuris ei intricetur & irretiatur; interim tamen cohæſio hæc *non* eſt *ſatis firma*, dum volatilis hic ſpiritus *per* propriæ quoque indolis, purius tamen, ab inflammabili plane depuratum, & magis fixum & *concentratum*, *acidum vitriolicum* rurſus inde expelli, & à Sale Alcalino fixo liberari poteſt, ex alia ratione nulla, quam *quia* bona ex parte *adhuc de principio inflammabili participat*, ex quo acidum iſtud adhuc volatile & ſubtile, per conſequens non ita purum & fixum exiſtit, ac oleum vitrioli rectificatum; ſi vero ejuſmodi ſpiritus ſulphuris volatilis tantum per aliquod tempus *libero aëri* exponitur, tum tenerum hoc inflammabile plane *diſſipatur* & *exhalat*, reſtante è contra itidem puro, fixo, & cum alio ex vitriolo, vel alumine, deſtillato acido, in

omnibus probis *æquali*, & in nulla re diverſo acido, quod tum cum Sale Alcali fixo, eadem ratione ac aliud purum, fixum, acidum vitriolicum, ſeſe quam *firmiſſime* commaritabit.

Poſt hoc acidum ſulphureum, vel vitriolicum, unit ſeſe noſtrum Sal Alcali fixum lubentiſſime *cum acido nitri*, ſeu ſpiritu nitri.

Et poſt hoc cum *acido ſalis communis*, ſeu *marini*.

Denique omnium debiliſſime *cum acidis vegetabilium*, ut acido aceti, tartari, ſpiritu lignorum, ſpiritu ſacchari, ſucco citri, vino Rhenano, *&c.*

Ex his igitur ſufficienter addiſci poteſt, qua ratione novæ ſemper combinationes Salis Alcalino-fixi cum ſale acido perpetrari, & idem alcali aliquoties debiliori acido per fortius rurſus ſurripi poſſit, ubi autem in *ſcala hac* non deviandum eſt. Nam ſi Sal Alcali fixum cum puro acido vitriolico combinatum foret, tum utique nullum acidum ex omnibus aliis potens eſſet, hoc Sal Alcali fixum ipſi rurſus eripere, quia nimirum hoc eſt fortiſſimum omnium, & omnia reliqua acida ſunt multo debiliora, utpote quæ ſingula contraria ex parte ab acido vitriolico ſeparari poſſunt.

Equidem D. D. *Stahl* etiam ad hoc medium quoddam demonſtravit, qua ratione Sal Alkali ex acido vitriolico rurſus liberari poſſit: verum hoc non fit mediante alio acido, ſed mediante additione principij inflammabilis, paucis per artificioſam ſulphuris formationem, per conſequens requiſito ad hoc labore in igne. Laudatus D.D. *Stahl* hunc mihi, *Anno* 1718, cum tunc temporis *Lutitiæ Pariſiorum* adhuc degerem, ſingularem honorem exhibuit, ut hujus liberationis cauſa *novum* plane *problema*, adiſtam ſulphuris geneſin plane non applicabile, in conſiderationem & ſolutionem per literas mihi proponeret; continetur autem ſequentibus verbis: *Qua ratione Sal Alcali fixum ab*

acido

acido vitriolico, prout utraque ſub figura cryſtallini tartari vitriolati apparent, in ſola cavitate manus, & unico in momento liberari poſſit ? Quæ momenta alias inaudita adhuc fuere, quod poſſibilia exiſtant, & forte ad hoc uſque tempus adhuc nemini præter laudatum D. *Stahlium* & me, vel cui prior forte iſtud manifeſtavit, nota erant. Dominus *Geoffroy* ſenior, profeſſor Chymiæ Pariſienſis, cui problema hoc tum temporis communicabam, Anno 1720, in hiſtoria academiæ ſcientiarum Gallicæ, illud imprimi curavit, & *putativam* hujus problematis *ſolutionem* annexuit; ſed vir hic quam amicus meus perdilectus, *rem plane non tetigit*, ſed penitus *aberravit*, dum deſideratam hanc Salis Alcalini ſeparationem, de formatione ſulphuris diu jam cognita & publice nota, explicat & applicat; hæc vero ſulphuris productio plane *non in unico momento, multo minus in ſola cavitate manus, ſed in crucibulo igne candente fit*, vel perpetrari debet.

Circa combinationes præcipitatorias, in quibus metalla ſoluta fuere, adhoc obſervandum eſt, quod Sal Akali fixum, durante unione cum acido, cadenti ad fundum *metallo alterationem* quandam *imprimat*, quæ non raro tantam mutationem involvit, ut talia cum ipſo præcipitata metalla perquam *difficulter in priſtinam malleabilem & fluxilem*, ſeu fuſibilem metallicam *formam* reduci ſeſe patiantur ; cujus rei (ut aliquid tantummodo adducam) etiam ferrum, quod ex commixtione vitrioli martis cum Sale Alcalino-fixo, in præparatione tartari vitriolati *Tachenij*, ad fundum cadit, ſufficiens teſtimonium perhibet.

Solent etiam noſtra *Salia Alkalina fixa*, præter memorata ſuperius corpora, per ſe adhuc diverſa *corpora metallica ſolvere*, quanquam pro hac intentione aliqualis cauſticitatis gradus requiratur.

 (*a*) Solvunt

(*a*) Solvunt proinde calcem *plumbi*, vel etiam plumbum tenerè raſum.

(*b*) Solvunt itidem ſubtilem raſuram *cupri*, vel etiam cuprum è vitriolo præcipitatum, quin & cuprum in viridi æris contentum.

(*c*) Sic & *ſtannum* ſolvunt;

(*d*) Ut & *regulum antimonij*, cum quo in calcem, imo etiam ultimatim, ſi fortiſſimo igne torquentur, plane in ſuccinei coloris vitrum convertuntur.

(*e*) *Ferrum* ſolvunt duplici ratione;

(1.) Vel dum cum nitro detonatur, & nitrum cauſticum ejus ope producitur;

(2.) Vel etiam, ſi puram ſolutionem Salis Alcali fixi ſolutioni martis cum ſpiritu nitri antea factæ infundo, prout hoc fuſius in Diſſertatione quadam ſub D. D. *Stahlio* de hac materia evolvi poteſt.

Quod ſi vero fixum noſtrum Sal Alcali *antea ſulphure* bene *imprægnetur*, tum *omnia metalla*, (quædam etiam ſemi-metalla) nimirum *cuprum*, *ferrum*, *plumbum*, *regulus antimonij*, imo etiam *ſtannum & aurum* cum ipſo diſſolvi poſſunt.

Denique, ut coronidem imponamus, adhuc meretur, quod Salia Alcalino-fixa, Salibus Alcalino-volatilibus *ſiccam conſiſtentiam* conciliant, quod corpora alcalino terrea non faciunt.

Solvunt etiam hæc fixa ſalia tam ex ſpiritibus urinoſis, quam ex ſpiritibus inflammabilibus quoque ſu-

perfluam, & ſuperficie tenus tantum adhærentem, *oleoſam* ſubſtantiam.

Quibus uſibus ulterius in Chymia adhiberi ſoleant, partim jam ſatis notum eſt, partim etiam occaſione variorum aliorum laborum jam à me pertractatum fuit, unde nec de his, nec de reliquis diverſis putatitiis, & injuria ſic æſtimatis, revera autem *falſis Salibus Alcalinis*, quippe de quibus adhuc unum & alterum monendum, ſi non fortaſſe totum capitulum annectendum eſſet, ulterius quicquam adducam ; ſed ne pertractatio hæc nimium excreſcat, finem hiſce imponam.

Berolin. Menſis
Sept. 8vo. 1725.

CASPAR NEUMAN,

Chymiæ practicæ Profeſſor Regius.

An

II. *Obſervations made at* Southwick, *in the County of* Northampton, Long. *Weſt from* London, 00 deg. 30 min. Lat. 51 deg. 58 min. *nearly, with a thirteen foot* Teleſcope, *whoſe Aperture was* 2.4 *Inches, and Charge* 2.5 *Inches, all by apparent Time. By the ingenious* George Lynn, *Eſq;*

1724. NOvemb. 8th, 7^h 37′ 7″ the firſt Satellite of *Jupiter* began to emerge: The ſame Day at 6^h 24′ 20″. The third Satellite began to immerge,

1725. *July* 31ſt, 10^h 43′ 20″. The third Satellite immerged, that is I quite loſt Sight of it, (at a little above a Semidiameter from *Jupiter*) but it began ſenſibly to abate of its Light above three Minutes before.

Auguſt 9th, 11^h 51′ 20″ I loſt Sight of the ſecond Satellite; but it began ſenſibly to abate of its Light, about two Minutes before.

Auguſt 18th, 9^h 25′ 50″. The firſt Satellite immerged very near *Jupiter*'s body.

The ſame Night, both my ſelf and Son plainly ſaw the Shadow of the third Satellite paſs over *Jupiter*'s body, like a ſmall black Patch, tracing along the Middle of his bright Belt, above the moſt Southern Black one, and was in his Axis, as near as I could gueſs by the Eye, at 10^h 25, or 30′.

N. B. We could ſee it for about the middle Half of its Track, but not near *Jupiter*'s Edges.

Octob. 11th, 6^h 31′ 45″. The third Satellite began to emerge, and was full three Minutes and a half, before it was at its greateſt Luſtre, which I could then

then well judge of, by comparing it with the firſt Satellite, which was juſt a little above it, but nearer *Jupiter*. It came out of the Shadow, about half a Diameter from *Jupiter*'s Edge.

Decemb. 26*th*, 5^h 51′ 12″ the ſecond Satellite began to emerge.

1725-6, *Jan.* 5*th*, 6^h 28′ 30″. The third Satellite began to emerge.

1724 *June* 23*d*, 10^h 15′ *Saturn* followed a Star (in *Senex*'s *Zodiack* but without any diſtinguiſhing Mark) 51″ and an half of Right Aſcenſion in Time, and declin'd from it South 40″.

June 25*th*, 10^h 0′ *Saturn* followed the ſame Star, 13″ of Right Aſcenſion in Time, and declined from it South, 3″ or 4″ only,

1725. *Decemb.* 17*th*, 8^h 0′ *Jupiter* preceded φ *Aquarii* 4″ and an half of Right Aſcenſion in Time, and declin'd from it South 11′ 45″.

N. B. When two of *Jupiter*'s Satellites are paſſing by one another, the one approaching, the other receding from him (if not too far diſtant from his Body) the Time, when they become equally diſtant from his Limb, may, by the Eye, be very nearly determined, eſpecially when the firſt and ſecond ſo paſs, as by Experience I have found by the above mention'd Glaſs, within leſs than half a Minute in Time, by the Agreement of two good Obſervers.

Therefore the taking the Time of thoſe Paſſages, I mean of the firſt and ſecond Satellites, would be of more uſe in ſettling the Longitude of Places, than the Eclipſes of any of the Satellites, except the firſt, by Reaſon of the Length of Time they take in emerging, or immerging, according to theſe Obſervations.

III. *An Extraordinary high Tide in the River* Thames *obſerv'd by Capt.* THO. JONES.

MARCH the 8th, 1725-6, The *Tide* in the *River Thames*, at *New Crane* in *Shadwell*, flow'd twenty Foot, five Inches and a half, taken by a Level, from that High-water Mark, to Low-water the next Morning, and was four Inches higher than has been known theſe fourty Years.

IV. *Obſervations upon the Tides in the River* Thames, *by Mr.* Henry de Saumarez.

THAT the Uſe of my Inſtrument call'd the * *Marine Surveyor* may yet farther appear, I ſhall here give ſome Experiments I made with it on the River *Thames*, in order to determine the Strength of the Tides of *Flood* and *Ebb*. Were the ſame to be done in the Channel, and on the Sea Coaſt of *Great Britain*, and mark'd in our Charts, I am humbly of Opinion, it would be of no ſmall advantage to our Commerce, and of conſequence a ſufficient Recommendation of the *Marine Surveyor*, if that alone were the Uſe of it.

I am induced the rather to be of this Opinion, in regard I am not inſenſible of the Dangers on the *Caſquets*, in the *Race* of *Alderney*, &c. where rapid Tides and Currents have occaſioned but too many to mourn the Loſs of Friends and Fortunes: As I dwell in the Neighbourhood of theſe Dangers, I have (in manifeſt hazard of my Life) ſurvey'd and taken correct Draughts not only of them, but of the Iſlands of *Guernſey*, *Sarck*, &c. And as I perſuade my ſelf they are as correct, as any Thing that has hitherto appear'd of this kind, it is my intent to publiſh them for the Good of the Publick.

* *Vid.* Philoſ. Tranſact. No. 391.

III. A TABULAR Account, ſhewing the Strength and gradual Increaſe and Decreaſe of the Tides of Flood and Ebb in the River *Thames*, as obſerv'd in *Lambeth* Reach, off of *Manchester* Stairs, and in the Middle of the River, with a new Inſtrument call'd the *Marine Surveyor*, on the 9*th* of *June*, 1720; It being then Full Moon, and conſequently a Spring Tide. The Movement of the *Machine* 14 Inches under Water.

FLOOD.

The Time of Flood.		The Depth of the River.		The Run of the Current in every 15 Min.	The whole Run of the Current to the Times expreſs'd in the firſt Column.	The ſame reduced to Statute Miles of 5280 feet, or 528 Revolutions of the *Machine*.		The Reduction into Engliſh maritime Miles of 6000 feet, or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts. Rev.	M.	Pts.	Rev.
	15	5		110	110		11			11
	30	6		590	700		70			70
	45	6	9	1100	1800		¼ 48		¼	30
1		7		1490	3290		½ 55		½	29
1	15	8		1870	5160		¼ 120		¾	66
	30	9		2230	7390		½ 79	1		139
	45	10		2500	9890	1	¼ 65	1	½	89
2		11	6	2660	12550	2	¼ 67	2		55
2	15	13		2730	15280	2	¾ 76	2	½	28
	30	14	8	2740	18020	3	¼ 86	3		2
	45	14	9	2720	20740	3	½ 94	3	¼	124
3		14	9	2570	23310	4	¼ 87	3	¾	81
3	15	14	10	2220	25530	4	¾ 45	4	¼	3
	30	14	9	1820	27350	5	66	4	½	35
	45	14		990	28340	5	¼ 62	4	½	134
	50	13	9	130	28470	5	¾ 75	4	¾	147

EBB.

The time of Ebb.		The depth of the River.		The Run of the Current in every 15 Min.	The whole Run of the Current to the Miles expressed in the first Column.	The same reduced to Statute Miles of 5280 feet, or 528 Revolutions of the Machine			The reduction into English maritime Miles of 6000 feet, or 600 Revolutions.		
H.	M.	Fe.	In.	Feet.	Feet.	M.	Fr.	Rev.	M.	Fr.	Rev.
											28
	30	12	3	1140	1420		1/4	10			142
	45	11	10	1900	3320		2	68		1/2	32
		11	4	2080	5400			12		3/4	190
1	15	11	2	2150	7550		1/4	92	1	1/4	2
	30	10	9	2120	9640	1	3/4	40	1	1/2	64
	45	10	4	2170	11810	2		125	1	3/4	131
2		10		2130	13940	2	1/2	74	2		44
2	15	9	6	2060	16000	3		16	2	1/2	100
	30	9	4	2040	18040	3	1/4	88	3		4
	45	9		2020	20060	3	3/4	26	3	1/4	56
3		8	9	1910	21970	4		85	3	1/2	97
3	15	8	6	1900	23870	4	1/2	11	3	3/4	137
	30	8	3	1910	25780	4	3/4	70	4		28
	45	8		1860	27640	5		124	4	1/2	64
4		7	9	1810	29450	5	1/2	41	4	3/4	95
4	15	7	3	1780	31230	5	3/4	187	5		123
	30	7		1690	32920	6		124	5	1/2	142
	45	6	6	1620	34540	6	1/2	22	5	3/4	4
5		6	3	1570	36110	6	3/4	47	6		11
5	15	6	3	1570	37680	7		72	6	1/4	18
	30	6		1570	39250	7	1/4	97	6	1/2	25
	45	6		1560	40810	7	1/2	121	6	3/4	31
6		5	9	1550	42360	8		12	7		36
6	15	5	6	1500	43860	8	1/4	30	7	1/4	36
	30	5	3	1460	45320	8	1/2	44	7	1/2	32
	45	5		1450	46770	8	3/4	57	7	3/4	27
7		4	9	1430	48200	9		68	8		20
7	15	4	6	1400	49600	9	1/4	76	8	1/4	10
	30	4	3	1380	50980	9	1/2	82	8	1/2	148
	45	4	3	1340	52320	9	3/4	84	8	3/4	232
8		4		1270	53590	10		79	8	3/4	109
8	5	3	10	420	54010	10		121	9		1
	10	3	11	410	54420	10	1/4	30	9		42
	15	4		400	54820	10	1/2	70	9		82
	20	4		380	55200	10	3/4	108	9		120
	25	4	2	300	55500	10	1/2	6	9	1/4	
	30	4	2	270	55770	10	1/2	33	9	1/4	27
	35	4	3	130	55900	10	1/2	46	9	1/4	40
	40			Stagnt.	Stagnant						

IV. A TABULAR Account, Shewing the Strength and gradual Increaſe of the Tides of Flood and Ebb in the River *Thames*, as obſerv'd in *Lambeth* Reach, off of *Mancheſter* Stairs, and in the Middle of the River, with a New Inſtrument call'd the *Marine Surveyor*, on the 18*th* of *June* 1720; It being then the laſt Quarter of the Moon and conſequently a Neap Tide. The Movement of the *Machine* 14 Inches under Water.

FLOOD.

The Time of Flood.		The Depth of the River.		The Run of the Current in every 15 Min.	The whole Run of the Current to the Times expreſs'd in the firſt Column.	The ſame reduced to Statute Miles of 5280 feet, or 528 Revolutions of the *Machine*.			The Reduction into Engliſh maritime Miles of 6000 feet, or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts	Rev.	M.	Pts	Rev.
	15	4		220	220			22			22
	30	4	3	520	740			74			74
	45	4	9	900	1640		1/4	32		1/4	14
1		5	5	1030	2670		1/2	3		1/4	117
1	15	5	9	1020	3690		1/2	105		1/2	69
	30	6	1	1160	4850		3/4	89		3/4	35
	45	7		1450	6300	1		102	1		30
2		7	9	1640	7940	1	1/2	2	1		44
2	15	8	1	1830	9770	1	3/4	53	1	1/2	77
	30	9		1920	11690	2		113	1	3/4	119
	45	9	6	2070	13760	2	1/2	56	2	1/4 1/2	26
3		10		2170	15930	3		9	2		
3	15	10	4	2070	18000	3	1/4	84	3		
	30	11	3	1960	19960	3	3/4	16	3	1/4	46
	45	11	4	1890	21850	4		73	3	1/2	85
4		11	9	1700	23550	4	3/4	111	3	3/4	105
4	15	11	6	1300	24850	4	1/2	109	4		85
	30	11		730	25580	4	3/4	50	4	1/4	8
	35	11		70	25650	4	3/4	57	4	1/4	15
	40			Stagnt.	Stagnant.						
	45	10	10	dit.	dit,						
	50	10	9	dit.	dit.						

EBB.

The Time of Ebb.		The Depth of the River.		The Run of the Current in every 15 Min.	The whole Run of the Current to the Times expreſs'd in the firſt Column.	The ſame reduced to Statute Miles of 5280 feet, or 528 Revolutions of the *Machine*.		The Reduction into Engliſh maritime Miles of 6000 feet, or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts Rev.	M.	Pts.	Rev.
	15	10	6	610	610		61			61
	30	10		1340	1950		¼ 63		¼	45
	45	9	9	1520	3470		½ 83		½	47
1		9	3	1650	5120		¾ 116		¾	62
1	15	9		1750	6870	1	¼ 27	1		87
	30	8	6	1730	8600	1	½ 68	1	¼	110
	45	8		1700	10300	1	¾ 106	1	½	130
2		7	9	1710	12010	2	¼ 13	2		1
2	15	7	3	1710	13720	2	½ 52	2	¼	22
	30	7	1	1710	15430	2	¾ 91	2	½	45
	45	6	9	1710	17140	3	130	2	¾	64
3		6	7	1680	18820	3	½ 34	3		82
3	15	6	4	1670	20490	3	¾ 69	3	¼	99
	30	6		1570	22060	4	94	3	½	106
	45	5	9	1500	23560	4	¼ 112	3	¾	106
4		5	8	1480	25040	4	½ 128	4		104
4	15	5	3	1440	26480	5	8	4	¼	98
	30	5	2	1430	27910	5	¼ 19	4	½	91
	45	5		1420	29330	5	½ 29	4	¾	83
5		5		1430	30760	5	¾ 40	5		76
5	15	4	10	1420	32180	6	50	5	¼	68
	30	4	6	1430	33610	6	¼ 61	5	½	61
	45	4	4	1420	35030	6	½ 71	5	¾	53
6		4	1	1380	36410	6	¾ 77	6		41
6	15	3	11	1360	37770	7	81	6	¼	27
	30	3	11	1340	39110	7	¼ 83	6	½	11
	45	3	10	1230	40340	7	½ 74	6	¾	134
7		3	10	1070	41410	7	¾ 49	6		91
7	15	3	11	530	41940	7	¾ 102	6	¼	144
	20	4		20	41960	7	¾ 104	6	¾	145
	25			Stagnt.	Stagnant.					
	35	4	3	dit.						

V. A New and Exact TABLE, Collected from ſeveral Obſervations, taken in four Voyages to *Hudſon's Bay* in North *America*, from *London*: Shewing the Variation of the *Magnetical Needle*, or Sea *Compaſs*, in the Path-way to the ſaid *Bay*, according to the ſeveral *Latitudes* and *Longitudes*, from the Year 1721, to 1725. By Mr. *Chriſtopher Middleton*.

Lat.		Long.		Vari.		Lat.		Long.		Vari.	
D.	M.	D.	M.	D.	M.	D.	M.	D.	M.	D.	M.
50	00	12	00	14	00	50	00	18	45	17	00
51	00	12	00	14	15	51	00	18	45	17	15
52	00	12	00	14	30	52	00	18	45	17	30
53	00	12	00	14	45	53	00	18	45	17	45
54	00	12	00	15	00	54	00	18	45	18	00
55	00	12	00	15	15	55	00	18	45	18	15
56	00	12	00	15	30	56	00	18	45	18	30
57	00	12	00	15	45	57	00	18	45	18	45
58	00	12	00	16	00	58	00	18	45	19	00
59	00	12	00	16	15	59	00	18	45	19	15
50	00	14	15	15	00	50	00	21	00	18	00
51	00	14	15	15	15	51	00	21	00	18	15
52	00	14	15	15	30	52	00	21	00	18	30
53	00	14	15	15	45	53	00	21	00	18	45
54	00	14	15	16	00	54	00	21	00	19	00
55	00	14	15	16	15	55	00	21	00	19	15
56	00	14	15	16	30	56	00	21	00	19	30
57	00	14	15	16	45	57	00	21	00	19	45
58	00	14	15	17	00	58	00	21	00	20	00
59	00	14	15	17	15	59	00	21	00	20	15
50	00	16	30	16	00	50	00	23	15	19	00
51	00	16	30	16	15	51	00	23	15	19	15
52	00	16	30	16	30	52	00	23	15	19	30
53	00	16	30	16	45	53	00	23	15	19	45
54	00	16	30	17	00	54	00	23	15	20	00
55	00	16	30	17	15	55	00	23	15	20	15
56	00	16	30	17	30	56	00	23	15	20	30
57	00	16	30	17	45	57	00	23	15	20	45
58	00	16	30	18	00	58	00	23	15	21	00
59	00	16	30	18	15	59	00	23	15	21	15

Lat.		Long.		Vari.		Lat.		Long.		Vari.	
D.	M.	D.	M.	D.	M.	D.	M.	D.	M.	D.	M.
50	00	25	30	20	00	50	00	34	30	24	00
51	00	25	30	20	15	51	00	34	30	24	15
52	00	25	30	20	30	52	00	34	30	24	30
53	00	25	30	20	45	53	00	34	30	24	45
54	00	25	30	21	00	54	00	34	30	25	00
55	00	25	30	21	15	55	00	34	30	25	15
56	00	25	30	21	30	56	00	34	30	25	30
57	00	25	30	21	45	57	00	34	30	25	45
58	00	25	30	22	00	58	00	34	30	26	00
59	00	25	30	22	15	59	00	34	30	26	15
50	00	27	45	21	00	50	00	36	45	25	00
51	00	27	45	21	15	51	00	36	45	25	15
52	00	27	45	21	30	52	00	36	45	25	30
53	00	27	45	21	45	53	00	36	45	25	45
54	00	27	45	22	00	54	00	36	45	26	00
55	00	27	45	22	15	55	00	36	45	26	15
56	00	27	45	22	30	56	00	36	45	26	30
57	00	27	45	22	45	57	00	36	45	26	45
58	00	27	45	23	00	58	00	36	45	27	00
59	00	27	45	23	15	59	00	36	45	27	15
50	00	30	00	22	00	50	00	39	00	26	00
51	00	30	00	22	15	51	00	39	00	26	15
52	00	30	00	22	30	52	00	39	00	26	30
53	00	30	00	22	45	53	00	39	00	26	45
54	00	30	00	23	00	54	00	39	00	27	00
55	00	30	00	23	15	55	00	39	00	27	15
56	00	30	00	23	30	56	00	39	00	27	30
57	00	30	00	23	45	57	00	39	00	27	45
58	00	30	00	24	00	58	00	39	00	28	00
59	00	30	00	24	15	59	00	39	00	28	15
50	00	32	15	23	00	50	00	41	15	27	00
51	00	32	15	23	15	51	00	41	15	27	15
52	00	32	15	23	30	52	00	41	15	27	30
53	00	32	15	23	45	53	00	41	15	27	45
54	00	32	15	24	00	54	00	41	15	28	00
55	00	32	15	24	15	56	00	41	15	28	15
56	00	32	15	24	30	57	00	41	15	28	30
57	00	32	15	24	45	58	00	41	15	28	45
58	00	32	15	25	00	59	00	41	15	29	00
59	00	32	15	25	15						

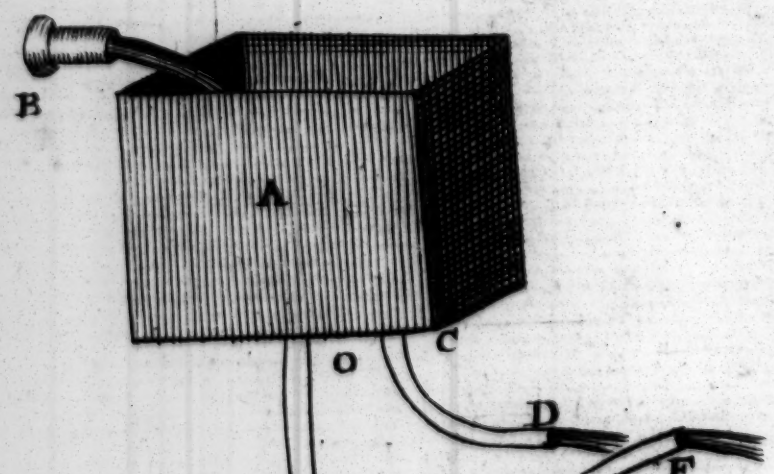
Lat.		Long.		Vari.	
D.	M.	D.	M.	D.	M.
50	00	43	30	28	00
51	00	43	30	28	15
52	00	43	30	28	30
53	00	43	30	28	45
54	00	43	30	29	00
55	00	43	30	29	15
56	00	43	30	29	30
57	00	43	30	29	45
58	00	43	30	30	00
59	00	43	30	30	15
51	00	46	00	29	00
52	00	46	00	29	15
53	00	46	00	29	30
54	00	46	00	29	45
55	00	46	00	30	00
56	00	46	00	30	15
57	00	46	00	30	30
58	00	46	00	30	45
59	00	46	00	31	00
52	00	48	30	30	00
53	00	48	30	30	15
54	00	48	30	30	30
55	00	48	30	30	45
56	00	48	30	31	00
57	00	48	30	31	15
58	00	48	30	31	30
59	00	48	30	31	45
53	00	51	00	31	00
54	00	51	00	31	15
55	00	51	00	31	30
56	00	51	00	31	45
57	00	51	00	32	00
58	00	51	00	32	15
59	00	51	00	32	30
60	00	51	00	32	45
54	00	54	00	32	00
55	00	54	00	32	15
56	00	54	00	32	30
57	00	54	00	32	45
58	00	54	00	33	00
59	00	54	00	33	15
60	00	54	00	33	30
61	00	54	00	33	45

Lat.		Long.		Vari.	
D.	M	D.	M.	D.	M.
54	00	57	00	33	00
55	00	57	00	33	15
56	00	57	00	33	30
57	00	57	00	33	45
58	00	57	00	34	00
59	00	57	00	34	30
60	00	57	00	35	00
61	00	57	00	35	30
55	00	60	00	34	00
56	00	60	00	34	30
57	00	60	00	35	00
58	00	60	00	35	30
59	00	60	00	36	00
60	00	60	00	36	30
61	00	60	00	37	00
57	00	63	00	35	00
58	00	63	00	35	30
59	00	63	00	36	00
60	00	63	00	36	30
61	00	63	00	37	00
62	00	63	00	37	30
59	00	66	00	37	00
60	00	66	00	37	40
61	00	66	00	38	20
62	00	66	00	39	00
63	00	66	00	39	40
60	00	69	00	41	00
61	00	69	00	41	40
62	00	69	00	42	20
60	00	72	00	40	00
61	00	72	00	42	00
61	40	72	00	42	40
62	00	78	00	43	00
63	00	78	00	44	00
63	50	78	00	46	00

Lat.		Long		Vari.	
D.	M.	D.	M.	D.	M.
61	00	75	00	38	00
62	00	75	00	43	00
62	50	75	00	45	00
63	00	81	00	43	00
64	00	81	00	46	00
62	00	82	00	39	00
63	00	82	00	44	00
61	00	84	00	33	45
62	00	84	00	40	00
63	00	84	00	42	00
60	00	86	00	30	00
61	00	86	00	33	00
62	00	86	00	35	00
59	00	88	00	28	00
60	00	88	00	28	40
61	00	88	00	29	20
57	00	90	00	24	00
58	00	90	00	24	30
59	00	90	00	25	00
57	00	94	00	23	00
58	00	95	00	22	30
59	00	95	00	21	00

From Long. 68 Degrees *to* 81. *is in* Hudſon's *Straits, where is the greateſt Variation, and the Compaſs will hardly Traverſe.*

Philos. Transact.
№. 393.



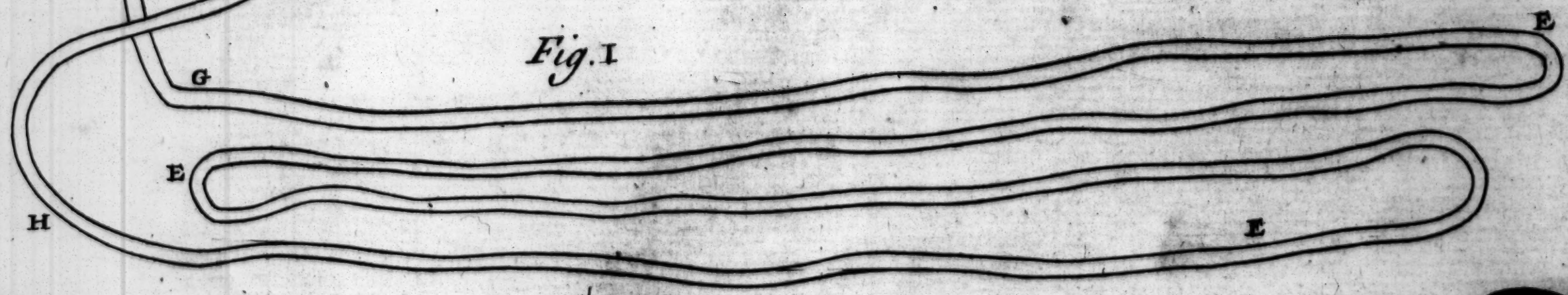


Fig. I

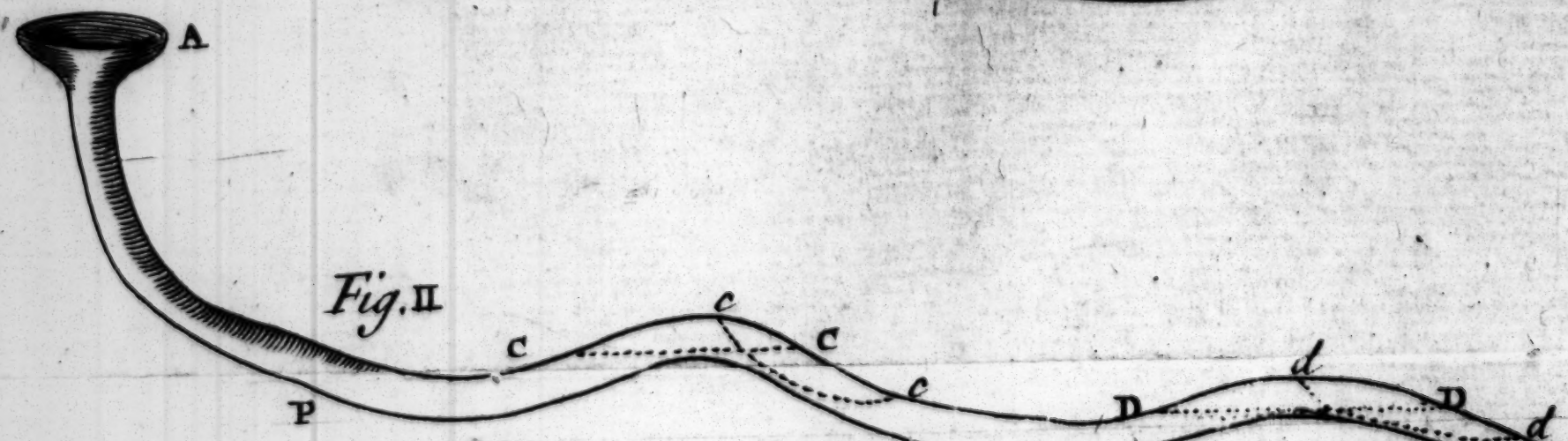


Fig. II

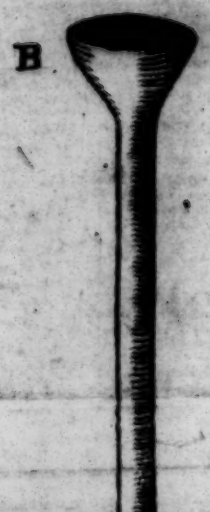
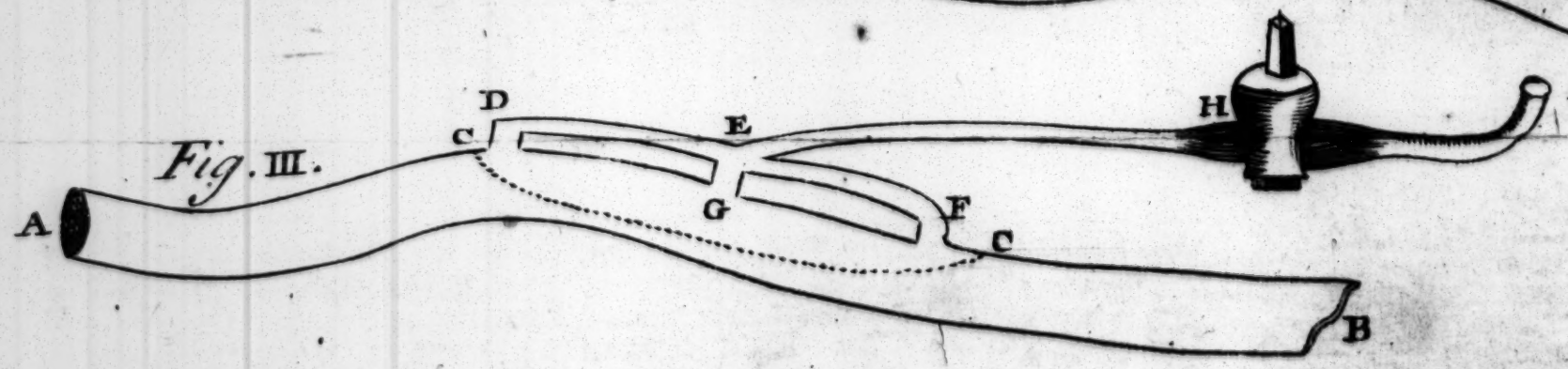
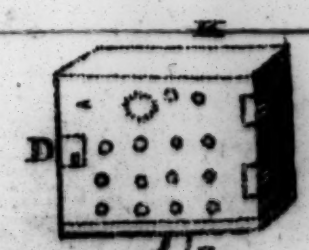


Fig. III.





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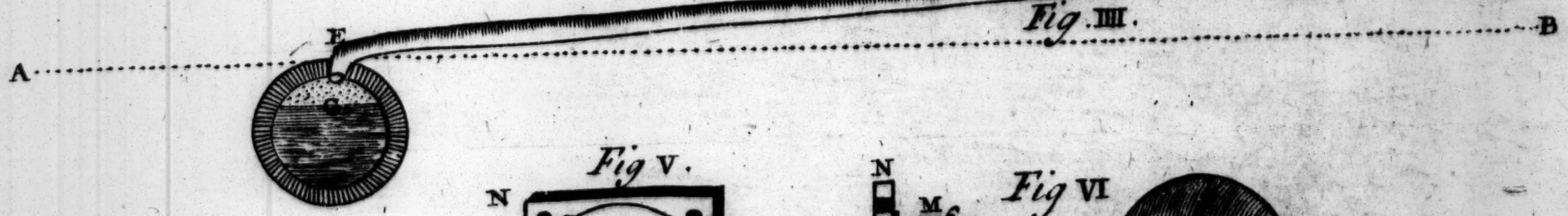


Fig V.

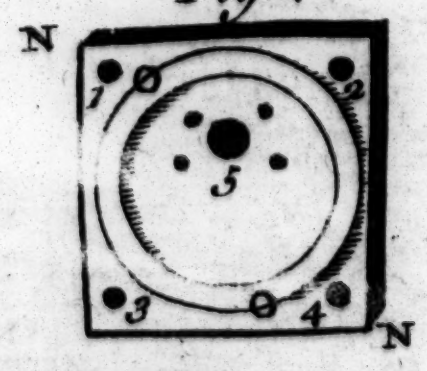


Fig VI

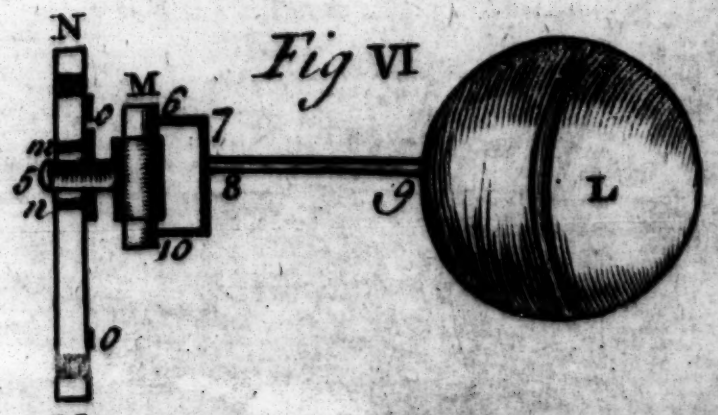


Fig VII

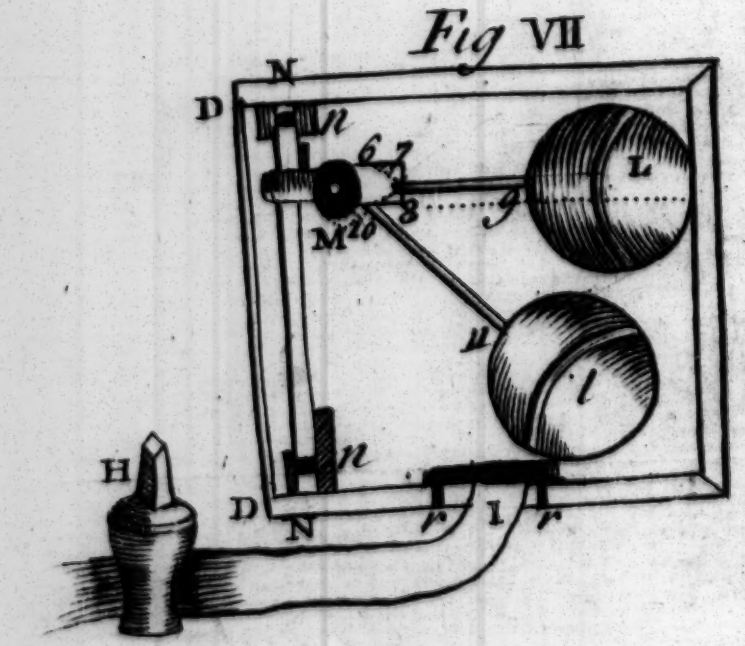
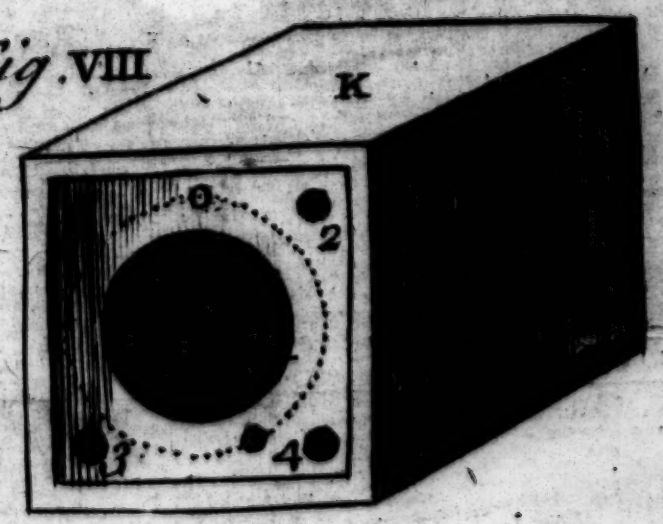


Fig. VIII



S. Parker Sculp.

VI. *An Account of ſeveral Experiments concerning the Running of Water in Pipes, as it is retarded by Friction and intermixed Air, ſome of which were made before the Royal Society on* Thurſday *the 5th of* May, 1726. *With a Deſcription of a new Machine, whereby Pipes may be clear'd of Air, as the Water runs along, without* Stand-Pipes, *or the help of any Hand. By the Rev.* J. T. Deſaguliers, LL. D. F. R. S.

HAVING found by ſeveral Experiments in ſmall, that thro' a long Pipe, Water wou'd not be diſcharg'd in the ſame Quantity by a great deal, as it wou'd be thro' a ſhorter of the ſame Bore, the Orifice being at the ſame Depth under the Surface of the Water in a Reſervoir; I made an Experiment upon a Pipe above 1000 Yards in length, and of $1\frac{1}{4}$ Inch Bore, at his Grace the Duke of *Chandos*'s Houſe at *Cannons*; and found that the Quantity of Water given was much leſs (I think $\frac{1}{11}$ leſs) than it ought to have been according to Monſ. *Mariotte*'s Rules; and that ſomething more than the Friction, on Account of the Length of the Pipe, had retarded the Water; which I found ſince to be Air confin'd in the eminent Parts of the Pipe. A full Account of this Experiment I publiſh'd in my Notes upon *Mariotte's Mouvement des Eaux*, in the *Engliſh* Tranſlation, ſome Years ago.

Conſidering this Matter again lately, I made the following Experiment. Fig. 1. *A* is a Veſſel containing a Cubic Foot in the Inſide, and always kept full

by means of the Pipe *B* running from a larger Veſſel. *C D*, is a ſhort Pipe of $\frac{1}{4}$ of an Inch Bore, two Foot in length, opening into the Bottom of the Ciſtern *A*, and whoſe Orifice *D* is always 10 Inches below the Bottom of *A*.

O G E E E H F, is another Pipe of the ſame Bore, whoſe Orifice *F* is likewiſe 10 Inches below the Bottom of *A*. This Pipe is 113 Yards long, lying along the Ground five Foot below *A*, except the depending Part *O G*, and the aſcending Part *H F*.

When *F* is ſtopped, and (*A* being kept full) the Water runs out at *D*, the Quantity of Water given is 19 Times more than when *D* is ſtopp'd, and the Water runs out at *F*.

The Air confin'd in ſeveral Parts of the long Pipe, is the chief Reaſon of this Difference.

In order to get rid of the Air, which lodging in the Pipe, contracts its Bore, and thereby leſſens the Quantity of Water, which is to be deliver'd at the Iſſue, I made ſeveral Experiments to find whereabouts the Air does lodge, the more eaſily to let it out; one of which was as follows.

I took a Glaſs Pipe as *A B* (Fig. 2.) of about one Inch in Diameter, 12 Foot in length from *P* to *P*; only the Parts *A P* and *P B* at the other End, were of Lead. Then pouring in Water at *A*, till it came up to *B* (ſtopping the End *G*) the Air lodg'd in the eminent Parts of the Pipe at the Places mark'd *C C*, *D D*, and *E E*: But when the Water was ſuffer'd to go out at *G*, the Air came forward towards *G*, and took up the Spaces *c c*, *d d*, and *e e*, contracting the Bore of the Pipe as before, but ſtood forwarder in the Pipe, ſo that it generally happen'd that the Space of Air began upon the upper part of the Eminence of the Pipe.

N. B. The Glaſs Pipe may be made of ſeveral Pieces join'd to each other, and to the Leaden Pipes

and Funnels, by Brass Ferrils and Elbows, turning in all manner of Angles. These are not represented here.

If the Velocity of the Water is very great, the Air will go even beyond the Eminence of the Pipe.

To let out the Air from the Conduct Pipes, which obstructs the Running of the Water, I recommend the Experiments which I made, and the Apparatus which I applied to a Wooden Conduct Pipe of nine Inches Bore, which runs a Mile and an half from the Water Engine at *York-Buildings* to a Reservoir near *Cavendish-square*; the Surface of the Water in the Cistern at the Water-house being sometimes 15, and sometimes 20 Foot above the Issue at the Reservoir.

Upon a Part of the Pipe, such as *AB* (Fig. 3.) I fix'd a Leaden Pipe *DF* of 2 Inches in the Bore, by means of 3 Ferrels, or short Communication-Pipes; the first at *D*, just beyond the Beginning of the Space *C C*, that us'd to be fill'd with Air in the running of the Water, the Second in the Middle of the Leaden Pipe, and the Third at the End of it; the Length of the Pipe itself being from 12 to 24 Feet, according to the Steepness of the Descent, the shortest Pipe being sufficient where the Descent is very quick. From the Middle of the Leaden Pipe above-mention'd (call'd a Rider, from its being laid along on the Main or Conduct Pipe) there goes another Leaden Pipe as *E H*, of the same Diameter, rising all the Way very gently from *E* to the Cock *H*, and so on to *I*; because, if there was the least Descent, Water wou'd lodge in it.

Now, when the Water runs from *A* to *B*, the first Ferril *D* will catch the Air as it runs, so as to let it out at *I*, if the Cock *H* be open, sometimes without going to *G* or to *C*. But if the Cock had not been open'd, till the Water had pass'd thro' the Part *A B* of the Pipe, the Air wou'd lodge in the Space *C C*, and be

 discharg'd

diſcharg'd upon the opening of the Cock. After the Cock has been ſhut, when no more Air comes, and Water ſucceeds, after ſome time, Air will extricate it ſelf out of the Water and come up to *CC*; or if it comes from the Parts of the Pipe towards *B*, it will riſe contrary to the Current of the Water quite up to *C*, and ſo go out at the Pipe *EH*, when the Cock is open'd again.

As after the firſt Diſcharge of the Air, it cannot be known when more Air is got into the Pipe, unleſs by opening the Cock, which wou'd require one Man to attend each Cock conſtantly, and occaſion a waſte of Water at every turn of the Cock, unleſs when Air happens to be in the Pipe; it was propos'd to contrive a Valve that ſhou'd open to let out the Air, and ſhut again when the Water came; and an inverted Braſs Clack or Valve ſhutting upwards, and falling down by its own Weight, with Cork fix'd to the Under-ſide of it to help it to riſe when the Water came, was mention'd as fit for the Purpoſe by ſome of the Perſons that I was talking with about it. But we rejected that Propoſal; becauſe, when ſuch a Valve has been ſhut ſome time, if Air ſhou'd extricate itſelf from the Water, it wou'd be denſe Air, whoſe Force being equal to that of a Pillar of Water 30, 60, 80 or more Feet in height, it wou'd keep the Valve ſhut as well as the Water did before, tho' the Air at firſt cou'd not ſhut the ſaid Valve.

At laſt, after ſeveral Thoughts, we contriv'd a Machine which exactly anſwers the Purpoſe, and is very ſimple; therefore it will be of general Uſe.

The Deſcription of it is as follows.

In the fourth Figure *G* is a Section of the Main or Conduct Pipe, with Water up to *G*, and Air above it, *AB* being an horizontal Line touching the Top of the ſaid Pipe: *EHI* is the Leaden Pipe deſcrib'd above,

and

andmark'd with the ſame Letters as in the third Figure, reaching from the Pipe in the Street to the Side of an Houſe, or to the Side of one of the Poſts that are ſet up to keep off Coaches from the Foot-way. The Machine is the Box *K* made of Caſt-Iron, fix'd to the Leaden Pipe at *I*, with a thin Door of Plate-Iron, moving on Hinges, and made to lock at *D*. This Box ſtands in the Street out of the way of Paſſengers, with its Bottom fix'd to a Plank in the Pavement, ſo as not to be damag'd by a ſmall Shock or any chance Blow.

The ſeveral Parts of the Machine are the following.

Figure 5; *N N* is an Iron Plate about an Inch thick, with 4 Holes at 1, 2, 3, 4, of about an Inch Diameter, quite thro' the Plate, to let thro' 4 Screws, ſuch as *a*; O O is a Face, or flat Ring rais'd out of the whole Stuff, and prominent about $\frac{1}{4}$ of an Inch, ground, or turn'd to a true Flat. 5, Is an Hole of about $1\frac{1}{2}$ Inch Diameter, to receive the Noſe of a Cock, which is put thro' it, ſtopping with a Shoulder or Flaunch ſcrew'd within the Circle O O by 4 other Screws mark'd with large Points round the Hole 5.

Fig. 6 *N N* is the ſame Plate ſeen Edge-wiſe.

M is the Air-Cock ſcrew'd to the ſaid Plate thro' the Flaunch of its Pipe at *m n*, having its Key 6, 10, faſten'd to a Rod of about $\frac{1}{2}$ an Inch Diameter of the Figure 6, 7, 8, 10, having a Shank one Foot long, 8, 9 join'd to a Buoy or hollow Copper Ball *L*, which Ball, when the ſaid Shank is in an horizontal Situation, keeps the Cock ſhut; but falling by its own Weight, when not ſuſtain'd by the Water, opens the Cock by means of the Rod 8, 9, as may be ſeen in Figure 7, where the Plate *N N* is ſcrew'd to the Box, and the prick'd Line *M L* ſhews the Surface of the Water coming into the Box thro' the great Cock and Leaden Pipe *H I*, ſo as to make the Ball *L* float with its Shank

in

in the horizontal Situation 8, 9; but when more Air comes in to drive the Water down the Pipe *I*, the Buoy will fall to *l*, and its Shank coming down to 10, 11, will open the Air Cock *M*, and let out the Air (be its Denſity what it will) till it be all diſcharg'd, and the Water is again got up to *M L*, and has rais'd up the Buoy to *L*.

N N is the Fore-part of the Box with its Hole, to which the Plate of Fig. 5. is ſcrew'd.

It is eaſily conceiv'd, that the Cock *H* muſt always be left open; that the End of the Pipe *I* is ſcrew'd to an Hole in the Bottom of the Box by means of Screws at *r r*; that there are oil'd Leathers at the Heads of all the Screws, and likewiſe upon the Plate *N N*, to make the Face O O of Fig. 5. apply it ſelf cloſe to the Fore-part of the Box *K* (Fig. 8.) which has an Hole at O O to take in the Buoy and Cock of Figure 6, the Screws at 1, 2, 3, 4, which have their Heads within the Box, and their Nuts ſuch as *b* (Fig. 5.) ſcrew'd on, when the Plate *N N* is apply'd; and that the whole Box, thus fitted is made Air-tight.

D in Figure 4. and *D D* in Fig. 7. repreſent an Iron Door, to cover the Mouth of the Air-Cock from external Injury, and is punch'd full of Holes to let out the Air freely.

This Machine, which from its Make, we call'd a *Jack in a Box*, will be uſeful wherever Water is to be convey'd a great way in Pipes; and ſince I was not the ſole Contriver of it, I ought in juſtice to thoſe that join'd their Thoughts with me, to acquaint the Publick, that the Box is the joint Invention of Mr. *Richard Jones*, Mr. *James King*, Mr. *Thomas Newcomen*, Mr. *Joſeph Hornblower*, his Operator, and my ſelf.

F I N I S.

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394.

Parker Sculpt

65-34.

A Scale of an Inch

3 6 9

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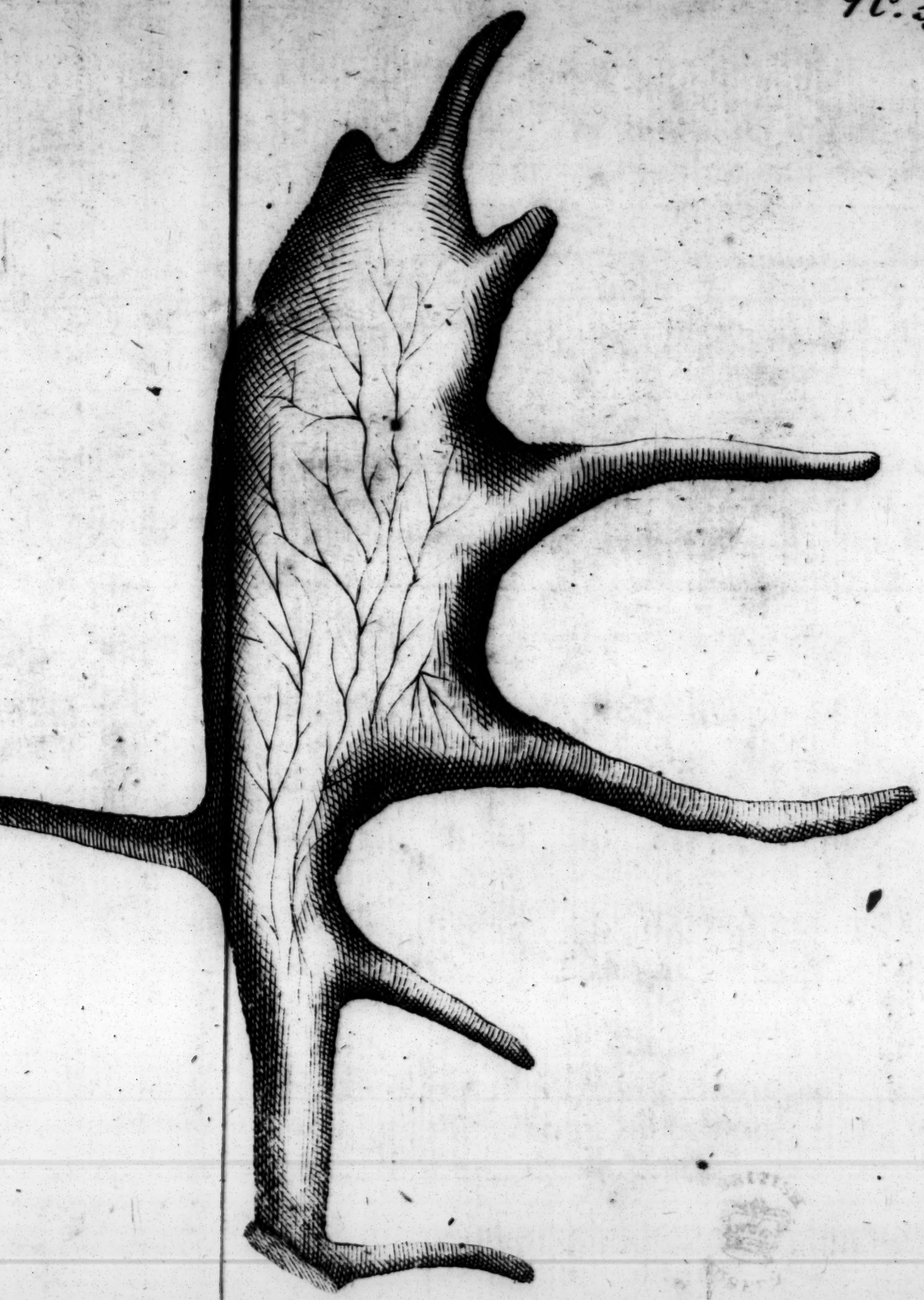

S. Parker Sculpt.

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PHILOSOPHICAL TRANSACTIONS

For the Months of *May*, *June*, and *July*, 1726.

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Jos: Banks

I. The

I. *The Longitude of* Lisbon, *and the Fort of* New York, *from* Wanſted *and* London, *determin'd by Eclipſes of the Firſt Satellite of* Jupiter. *By the Reverend Mr.* James Bradley, M. A. Aſtron. Prof. Savil. R. S. S.

SOME curious Aſtronomical Obſervations having lately been communicated to this Society from *Lisbon*, among which were ſeveral Eclipſes of the firſt Satellite of *Jupiter* *; I was willing to examine whether I had made any at *Wanſted* which tallied with them, that by comparing ſuch together, the true Difference of Longitude between thoſe Places might be found. But looking over my Obſervations of the firſt Satellite, made laſt Year and the beginning of this, I meet only with Two Emerſions that were obſerved the ſame Night both at *Lisbon* and *Wanſted*. There are others, indeed, made within a few Days of each other, which may likewiſe be made uſe of to determine the Difference of Longitude; but not with the ſame degree of Certainty, by reaſon of the irregular Motion of the Satellite; which I preſume, chiefly ariſes from the Gravity of the other Satellites towards it. For altho' the Effect of the Influence that the Satellites have on each other, is moſt remarkable in the Second, whoſe Motion will ſometimes be accelerated or retarded thereby, as much as amounts to 30 or 40 Minutes in time, in the ſpace of about ſeven Months, or in half the Period in which the three innermoſt Satellites return, to have nearly the ſame Poſition with reſpect to themſelves, and the Shadow of *Jupiter*; yet the firſt

* *Vide* Page 90.

ſeems alſo liable to Inequalities that cannot well be accounted for, but from ſome ſuch Cauſe as is before-mentioned, the effect of which will not eaſily be reduced to any Rule, but from a long and exact Series of Obſervations. And till ſome better and more certain Rule can be found out, we may ſuppoſe, that the Effect produced by this Cauſe, is, during ſmall Intervals, proportionable to the time. On this Suppoſition I have compared ſome Obſervations with others not made the ſame Nights; and the reſult is nearly the ſame as in thoſe which were obſerved at the ſame time in both Places, as will appear by the following Particulars.

The Immerſion of the Firſt Satellite was obſerved at *Wanſted* with Mr. *Hadley's* Reflecting Teleſcope on *Auguſt* 4, N. S. 1725, about 45″ after the time of the Immerſion, as calculated from my Tables. By another Obſervation made *Auguſt* 29, N. S. the true Immerſion preceded the Calculation from the ſame Tables 1′ 10″. So that in 25 Days the Satellite's Motion was accelerated as much as anſwer'd to 1′ 55″ in time. Suppoſing therefore the Acceleration to have been in the ſame proportion between *July* 28, and *Auguſt* 4, N. S. then the true Immerſion *July* 28, N. S. would have happen'd at *Wanſted* about 1′ 15″ after the time by the Tables, which make the Immerſion at 12 h. 48′ 45″ App. Time. The true Immerſion therefore was at *Wanſted July* 28, N. S. 12h. 50′ 0″ App. Time; and at *Liſbon* 'twas obſerved at 12h. 12′ 26″ App. Time, the Difference being 37′ 34″.

September 28, N. S. the Firſt Satellite was ſeen emerging in the Reflecter at *Wanſted* 3′ 50″ ſooner than the Tables make the Emerſion; and by the Mean of two more Obſervations made at the ſame Place, and with the ſame Teleſcope, on the 14*th* and 16*th* of *October*, N. S. the true Emerſion preceded the Calcu-

lation

lation 4′ 30″. We may therefore from hence conclude, that on *Sept.* 21, N.S. the true Emerſion at *Wanſted* preceded the Calculation by the Tables about 3′ 35″, and that the true Emerſion there was at 12h. 1′ 15″ *Apr.* 1; but this Emerſion was obſerved at *Lisbon* at 11h. 24′ 55″, the Difference being 36′ 20″.

The Obſervations at *Wanſted* being made with Mr. *Hadley*'s Reflecting Teleſcope (by which one may ſee the Firſt Satellite near ¼ of a Minute ſooner when 'tis Emerging, than in a Refracting Teleſcope of 15 Feet, and the contrary when 'tis Immerging) there ought to be ſome Allowance made on account of different Teleſcopes made uſe of at *Lisbon* and *Wanſted*, by deducting 10 or 15″ from the Difference of Time collected from the Immerſions, and adding as much to the Difference deduced from the Emerſions. Such Correction being made, the Difference of Meridians by the Immerſion obſerved *July* 28, will be 37′ 20″, and by the Emerſion *Sept.* 21, 36′ 35″.

The Emerſion obſerved at *Lisbon*, *Decemb.* 8, N.S. at 8h. 32′ 40″ Apparent Time, was likewiſe ſeen at *Wanſted* in a 15 Foot Tube at 9h. 10′ 5″ Apparent Time, the Air being a little hazy, which may probably make the Difference 37′ 25″ a little too great.

The Emerſion ſeen at *Lisbon Jan.* 16, 1726, N.S. at 6h. 51′ 10″, which ſeems accompanied with Circumſtances that argue its Exactneſs, was likewiſe very well obſerved at *Wanſted* in a 15 Foot Tube at 7h. 28′ 22″ Apparent Time, the Difference being 37′ 12″.

Theſe are the only Obſervations among thoſe which were laſt communicated, that I could compare with any degree of Certainty with my own: But I find others printed in the *Philoſoph. Tranſact.* N°. 385, which were likewiſe made by the ſame curious Perſons, who obſerved an Emerſion of the Firſt Satellite at *Lisbon*

September 2, 1724, N.S. at 9h. 36′ 57″. This was ſeen alſo at *Wanſted* in the Reflecter at 10h. 13′ 28″ Apparent Time. Hence, allowing for the different Teleſcopes, the Difference of Meridians is 36′ 45″.

This Emerſion at *Wanſted* preceded the Calculation by the Tables 4′ 40″: And another Emerſion obſerved with the ſame Teleſcope on *Sept.* 18, N.S. preceded the Calculation 5′ 10″. We may therefore ſuppoſe, that on *Sept.* 9, N.S. the true Emerſion at *Wanſted* preceded the computed about 4′ 52″. The Emerſion that Day by the Tables was at 12h. 15′ 34″ App. Time; therefore the true Emerſion at *Wanſted* was at 12h. 10′ 42″. At *Lisbon* twas obſerved at 11h. 34′ 26″. So that allowing for the Difference of Teleſcopes, the Difference of Meridians by this Obſervation is 36′ 30″.

The Mean of all theſe Differences is about 36′ 58″, from which ſubſtracting 28″ for the Difference of Meridians between *London* and *Wanſted*, the remainder will be the Difference of Meridians between *London* and *Lisbon*, viz. 36′ ½ = 9° 7′ ½, *Lisbon* being ſo much to the Weſtward of *London*. This Difference of Longitude is about 5′ ½ greater than what is determined in the forementioned *Tranſaction*: But as the Gentlemen to whom we are indebted for theſe Obſervations, have given us hopes that they will continue to make and communicate more, we need not doubt but their exact Care and Diligence will ſoon enable us to judge yet more nicely of the true Situation of thoſe Cities with reſpect to each other.

The ſame *Tranſaction* containing ſome Obſervations of Eclipſes of the ſame Satellite made in the Fort of *New York*, communicated by his Excellency *William Burnet*, Eſq; Governor of *New York*, I ſhall take this Oppor-

Opportunity of determining the Longitude of that Fort more exactly than it can be ſuppoſed to be there done, by the bare Compariſon of the Obſervations with the Tables; having two Obſervations made at *Wanſted*, which tally with two made at *New York*, on *Aug.* 25, and *Sept.* 10.

By the Obſervation made *Aug.* 25, 1723, O. S. which is eſteemed the moſt diſtinct and beſt, the Satellite Emerged at 9h. 35′ 14″ by the Clock, which went about 1′ ¾ too faſt for the Apparent Time at the Emerſion, as appears by the Altitudes of the Sun's Limb taken the Morning before and after the Obſervation; ſo that the Emerſion at *New York* was at 9h. 34′ Apparent Time; that is, 9h. 32′ 20″ Mean Time.

Auguſt 27, 8h. 57′ 40″ Mean Time, the Satellite was ſeen emerging at *Wanſted* in the Reflecter; and *Sept.* 12, 7h. 17′ 15″ M. T. 'twas ſeen emerging again in the ſame Teleſcope: So that in 15d. 22h. 19′ 35″ there were 9 Emerſions; and the Interval between each was about 1 d. 18h. 28′ 50″. This ſubſtracted from the Time of the Emerſion obſerv'd at *Wanſted Auguſt* 27, will give the true Emerſion at *Wanſted* on *Auguſt* 25, 14h. 28′ 50″ M. T. that is, 4h. 56′ 30″ later than it was obſerved at *New York.*

September 10, 8 h. 0′ 10″ by the Clock, another Emerſion was obſerved at *New York.* From the Altitudes of the Sun's Limb taken the Morning before, I compute the Error of the Clock at the time of the Emerſion to be 1′ 10″, and that the Emerſion happen'd at 7h. 59′ App. T. that is, 7h. 51′ 52″ Mean Time at *New York.* But ſubſtracting the forementioned Interval of 1 d. 18 h. 28′ 50″ from the Time of the Emerſion obſerved at *Wanſted September* 12, 7h. 17′ 15″ M. T. we ſhall have the time of the true Emerſion at *Wanſted* on *Sept.* 10, at 12h. 48′ 25″ M. T.

which

which is 4h. 56′ 33″ later than 'twas obſerved at *New York*. The Difference therefore of Meridians between *Wanſted* and *New York*, allowing about 15″ for the Difference of Teleſcopes, is about 4h. 56′ 45″, and between *London* and *New York*, 4h. 56′ ¼. So that the true Longitude of *New York* from *London* is 74° 4′ Weſt.

II. *Obſervationes Aſtronomicæ habitæ* Ulyſſipone, *Anno* 1725, *& ſub init.* 1726, *à Rev. P.* Johanne Baptiſta Carbone, *Soc. Jeſ. Communicante* Iſaaco Sequeyra Samuda, *M.D. R.S.S. Coll. Med. Lond. Lic.*

Temp. Ver. correct. à Meridie.

RArò cælum hoc anno nubibus expers contemplari licuit. Tunc verò vel maximè turbatum ſenſimus, cùm aliquid ſpectatu dignum propiùs immineret; ut meritò crederem, omnes nobis hoc anno obſervationes Aſtronomicas fuiſſe interdictas. Perpaucas tandem habere datum eſt circa conſuetas intimi Jovis Satellitis Eclipſes, quas hìc ſubnecto, Lunari Eclipſi, die 21 Octobris, Martiſque tranſitu per Lunam, die 18 Septembris, omninò inobſervatis.

Mens.	*Dies.*		*H.*	*M.*	*S.*
Jul.	28.	Immergi viſus eſt in umbram Jovis veram, teleſcopio conſueto Joſephi Campani palmorum Rom. 30. Ceperat verò debilitari lumen,	12	12	26
			12	11	35

Emerſit

Menſ.	*Dies.*		*H.*	*M.*	*S.*
Sept.	12.	Emerſit ab umbra vera Jovis, cælo ſatis ſereno; verùm ob Jovis cum Sole oppoſitionem, quæ ſeptem ante diebus contigerat, adeò Planetæ diſco proximus erat Satelles, ut ab ejus nimia claritate offuſcari aliquantulùm potuerit in primo ſui ab umbra egreſſu; ac proinde dubitari poteſt de aliquot ſecundis,	15	0	10
	14.	Cepit ab umbra emergere ——	9	28	7
		Integrum verò lumen recuperavit ——	9	29	0
	21.	Initium Emerſionis ab umbra vera ——	11	24	55
		totalis recuperatio luminis —— ——	11	26	0
Oct.	23.	Initium Emerſionis —— —— ——	8	11	10
		totalis recuperatio luminis —— —— ——	8	12	10
Nov.	8.	Viſus eſt Satelles clareſcere in penumbra; cum aliqua tamen incertitudine de vero initio emerſionis, ob aeris à vento trepidationem. Sed dubium fortaſſe quoad pauca tantùm ſecunda.	6	30	4
	15.	Diſtinctè obſervatum eſt initium Emerſionis. Recuperatio integra luminis —— ——	8	24	50
			8	25	50
Dec.	8.	Initium Emerſionis —— —— —— ——	8	32	40
		Totalis luminis recuperatio, —— ——	8	33	30

Anno

Anno 1726.

Menſ.	*Dies.*		*H.*	*M.*	*S.*
Jan.	9.	PRimò clareſcere viſus eſt, ſed luce tenuiſſima, ob aeris claritatem à crepuſculis, ac proinde non ſatis conſtat de vero initio emerſionis, ſaltem quoad ſecunda.	4	58	50
	16.	Satis clarè ac diſtinctè primò emergere viſus eſt, aere omninò pacato, ac ſereno ——	6	51	10
		totalis verò luminis recuperatio	6	52	15

Has tantùm circa primum Satellitem obſervationes habere nobis contigit ante Jovis cum Sole conjunctionem. Jam verò tempus adeſt, novas obſervandi Immerſiones; quas pro more noſtro tum diligenter obſervare, tum fideliter adnotare conabimur.

De Poli Elevatione Ulyſſipone.

QUÆ duo ad rem noſtram plurimùm facere arbitrati ſumus; quæque jam tum cùm ad hanc urbem primò appulimus, explorare contendimus, utpote ſupputationum omnium Aſtronomicarum præcipuum fundamentum, hujus, inquam, Clariſſimæ, atque Ampliſſimæ Urbis geographicam Longitudinem, ejuſque Latitudinem, ea ſanè quam-proximè nos eſſe aſſecutos exiſtimaverim. Primum, quod in ſpeciem difficiliùs videtur, circa finem primi anni à noſtro huc adventu, aſſequuti ſumus, comparatis nempe nonnullis obſervationibus,

nibus, quas hìc diligenter habueramus circa Eclipſes intimi Jovis ſatellitis cum habitis Romæ à Cl. V. Franciſco Blanchino. Deinde altero anno idem confirmavimus, tum novis ac repetitis obſervationibus circa eundem Satellitem, tum obſervationibus Eclipſis lunaris, quas contulimus cum habitis in Obſervatorio Regio Pariſienſi per Cl. V. Dominum Maraldum Regium Aſtronomum. Invenimus autem Ulyſſiponis Longitudinem, Grad. 8. Min 33 ; ſuppoſitâ tum Romanâ, tum Regii Obſervatorii Longitudine, pluribus anteactis experimentis, jam certâ. Obſervatorii ſiquidem Longitudo, ut Regiæ Academiæ monumenta perhibent, eſt Grad. 20. Min. 0. Differentia verò inter hunc Meridianum, ac Regii Obſervatorii, ut aliàs adnotavi, eſt Grad. 11. Min. 27, quibus occidentalior eſt Ulyſſipo; his igitur ſubtractis à Gradibus 20, remanet hujus Meridiani Longitudo ſupradicta Gr. 8. Min. 33.

Secundum verò, nempe hujuſce urbis Latitudinem, quam facilè quiſque aſſequi poſſe videtur, cùm innumeræ circa illam inſtitui poſſint interdiu, noctuque obſervationes, non adeò nobis opportunum fuit illam citiùs explorare ; inſtrumenta ſiquidem, quæ hìc invenimus, quæque etiam nobiſcum tulimus, etſi ſatis apta ad gradus ac minuta prima præter propter dignoſcenda, non tamen ad minuta eadem certiùs exploranda, multoque minùs ad ſecunda inveſtiganda (quæ ſanè Aſtronomis contemnenda non ſunt) opportuna eſſe videbantur. Atque hoc magìs, quod non una erat de hujuſce urbis Latitudine ſententia, quoad minuta prima ; quarum quidem nonnullæ propiùs veritati accedebant, ut ex noſtris quoque obſervationibus inferre licebat ; verùm nulli fidere certò poteramus. Duas tantùm ſententias in medium profero, quarum ſingulæ plurimùm ponderis apud prudentes habere poſſent ; ni diſcordia inter utramque earum fidem imminueret. Alte-

ra igitur ſententia eſt clariſſimi ac doctiſſimi viri Emanuelis Pimentel Regii Coſmographi, in mathematicis apprimè verſati, qui multis ac repetitis obſervationibus per umbram rectam Gnomonis, cujus altitudo pedum 16, ſe inveniſſe teſtatur hujus Poli altitudinem 38° 48′ 20″, quod in libello quodam MS. ejuſdem authoris legi, in quo & ipſas obſervationes fusè adnotaverat. Altera verò ſententia eſt Regiæ Pariſienſis Academiæ ex obſervationibus domini Couplet, qui Ulyſſiponem venit Anno 1697, ubi aliquot inſtituit obſervationes ad Meridianorum differentiam Ulyſſiponenſis ſcilicet ac Regii Obſervatorii, necnon hujuſce urbis latitudinem inveniendam. Hanc autem ſe inveniſſe fatetur 38° 45′ 25″. Ut igitur certi aliquid in hac re deprehendere poſſemus, expectandum tantiſper exiſtimavimus, dum aptiora Pariſiis inſtrumenta reciperemus; ubi juſſu ac munificentia ſereniſſimi Regis noſtri tum multa alia conficiebantur à peritiſſimis artificibus inſtrumenta, tum in primis Quadrantes duo Aſtronomici, quorum alter quinque, alter trium pedum Pariſinorum, nec non Sextans totidem pedum. Hæc jam inde recepimus, & multiplici experimento ad trutinam revocavimus; nec ſanè quidquam ſenſibile in iis corrigendum invenimus, niſi quod facillimè corrigi poteſt, & plerumque obſervatorum curæ corrigendum relinquitur, dioptras nempe Teleſcopicas ad rectum ſitum reducere. Id facilè præſtitimus, ſtatimque prædictis inſtrumentis uti cepimus, ad prædictam Poli altitudinem inquirendam.

Plurimas ſanè inſtituimus obſervationes; quarum tamen aliquot hìc ſubnecto circa Solis altitudines, præſertim Meridianas, vel ſextante, vel quadrante Aſtronomico trium pedum habitas, poſtmodùm miſſurus alias tum prædictis, tum etiam quadrante murali quinque pedum habendas vel circa Solem, vel circa reliqua Aſtra. Juvat verò tum obſervatas altitudines Solis

ſub-

ſubnectere, tum integras etiam ſupputationes, unde Poli altitudo deducta eſt.

Obſervationes Altitudinum Solis Meridianarum ad Poli Elevationem inveſtigandam Ulyſſip.

SEquentium obſervationum aliæ habitæ ſunt in Collegio divi Antonii Magni, aliæ in Obſervatorio Palatii Regii; quæ ſanè loca, cùm in eodem ſint Meridiano, quidquid inter ſe differunt, in ſola differunt Latitudine: At verò tanta non eſt differentia, ut ejus habenda ſit ratio in his obſervationibus, quibus hujuſce urbis Elevationem non adeò exactè exploratam volo, ut ſecunda quoque Graduum inventa affirmem. Diuturnioribus hæc opus habent experimentis; quippe ab innumeris pendent circunſtantiis, quæ non æquè ſemper concurrunt. Seriùs quidem id nos aſſecuturos arbitror.

Die 24. *Novembris* 1725.

	°	′	″
Altitudo Limbi ſuperioris Solis in Merid. quadrante Aſtronomico obſervata -----	30	56	20
Refractio propria hujus altitudinis ex tabulis Cl. V. Edmundi Halleii ———		1	28
Altitudo correcta ejuſdem Limbi ———	30	54	52
Parallaxis Solis ———			4
Altitudo vera Limbi ſuper Solem ———	30	54	56
Semidiameter Solis Appar. ———		16	18

	°	′	″
Altitudo vera centri Solis	30	38	38
Declinatio Solis Auſtral.	20	38	59
Altitudo Æquatoris	51	17	37
Complementum, ſeu Latitudo Ulyſſiponis	38	42	23

Die 5. Decembris.

Altitudo Meridiana Limbi ſuperioris Solis, ſextante obſervata	29	8	10
Refractio propria hujus altitudinis		1	35
Altitudo ejuſdem Limbi correcta	29	6	35
Parallaxis Solis			4
Altitudo vera Limbi ſuperioris Solis	29	6	39
Semidiameter Solis		16	20
Altitudo vera Centri Solis	28	50	19
Declinatio Solis Auſtralis	22	27	7
Altitudo Æquatoris	51	17	26
Complementum, ſeu Poli elevatio	38	42	34

Die 6. Decembris.

Altitudo Limbi ſuperioris Solis ſextante obſervata	29	1	0
Refractio propria hujus altitudinis		1	36
Altitudo correcta ejuſdem Limbi	28	59	24
Parallaxis Solis			4

Alti-

	°	′	″
Altitudo vera Limbi ſuperioris	28	59	28
Semidiameter Solis		16	21
Altitudo vera Centri Solis	28	43	7
Declinatio Solis Auſtralis	22	34	24
Altitudo Æquatoris	51	17	31
Complementum, ſeu Latitudo Ulyſſip.	38	42	29

Die 29. *Decembris.*

	°	′	″
Altitudo Limbi ſuperioris Solis quadrante obſervata	28	20	22
Refractio propria hujus altitudinis		1	39
Altitudo correcta Limbi ſuperioris Solis	28	18	43
Parallaxis Solis			4
Altitudo vera Limbi ſuperioris Solis	28	18	47
Semidiameter Solis		16	21
Altitudo vera Centri Solis	28	2	26
Declinatio Auſtralis	23	14	57
Altitudo Æquatoris	51	17	23
Complementum, ſeu Latitudo Ulyſſip.	38	42	37

Die 8. Januarii 1726.

	°	′	″
Altitudo Limbi inferioris Solis ſextante obſervata	28	47	10
Refractio propria hujus altitudinis		1	37
Altitudo correcta Limbi inferioris Solis	28	45	33
Parallaxis Solis			4
Altitudo vera Limbi inferioris Solis	28	45	37
Semidiameter apparens Solis		16	21
Altitudo vera Centri Solis	29	1	58
Declinatio Auſtralis Solis	22	15	42
Altitudo Æquatoris	51	17	40
Complementum, ſeu Latitudo Ulyſſip.	38	42	20

Die 9. Januarii duas Solis altitudines ſextante Aſtronomico obſervavimus ante Meridiem, totidemque poſt Meridiem, alteras alteris reſpondentes, in verticalibus ſcilicet à Meridiano æquidiſtantibus; quod ut accuratiùs fieret, addita ſunt altitudinibus pomeridianis ſcrupula convenientia, quæ nimirum ex declinatione Solis, utcunque minori, refundi debebant in ipſam Solis verticalem altitudinem. Has quidem obſervationes eatenùs inſtituimus, ut noſtri horologii pendulo inſtructi vel minimam à tempore vero diſcordiam deprehenderemus, ſimulque meridianas lineas, quibus non unis utimur in hoc noſtro Collegio, iterùm atque iterùm ad trutinam revocaremus. Utræque verò obſervationes inter ſe collatæ,

adeò

adeò exactè consensere in eadem differentia ostendenda, ut de illarum rectitudine non nisi temerè dubitarem. Opportunè igitur his ipsis observationibus usuros nos duxi ad Poli quoque altitudinem explorandam; his tribus nempe cognitis, altitudine Solis, ejusdem declinatione, & hora diei.

	☉	′	″
Altitudo Solis vera —— ——	20	36	18
Ejusdem declinatio Australis ——	22	8	11
Tempus verum observatæ altitudinis	9h.	37	26

Ex his igitur, per calculos Trigonometricos, quos inivimus (nec tamen hìc apponere opus est) resultat Poli elevatio Ulyssipone, 38° 42′ 24″.

Iterùm, ex secunda observatione matutina,

	°	′	″
Altitudo vera Solis —— —— ——	23	25	47
Ejusdem declinatio Australis ——	22	8	10
Tempus verum observatæ altitudinis	10h.	4	41

Ex quibus pariter per Trigonometriam resultat Poli elevatio Ulyssip. 38° 42′ 25″.

Ex observationibus pomeridianis, in quibus penè omnia sunt eadem atque in observationibus matutinis, eadem quoque inferri debebat altitudo Poli, ac proinde novis calculis non fuit opus.

Jam verò ex omnium observationum complexu, inferre hactenus licet Latitudinem Ulyssiponensem in hoc Collegio divi Antonii, aut etiam in Palatio Regio observatam non excedere 38° 43′, nec minorem esse 38° 42′; propiùs verò accedere ad 38° 42′ 30″. Quod sanè quamprimum novis atque iteratis observationibus certiùs innotescet.

Aliorum diſſidium in hac Elevatione aſſignanda, vel ex aliquo inſtrumentorum vitio (quod non facilè noſtris contingere potuit, cum & plura ſint, & grandiuſcula, nec uno ab artifice elaborata, ac tandem ſæpiùs adhibita in idem ſemper conſpiraverint) vel ex locorum diverſitate, in quibus obſervationes habitæ ſunt, oriri putaverim. Eſt enim Ulyſſipo ſatis ampla ; & à Borea ad Meridiem plus una extenditur Leuca, quæ quidem diſtantia trium, vel quatuor minutorum parere poſſet diſcordiam. Accedit ad hæc non exigua difficultas, quam quiſque, vel peritiſſimus, experiri ſolet in determinandâ extremitate umbræ veræ, eâdemque à penumbrâ ſecernendâ; quam ſanè difficultatem non facilè declinaverit clariſſimus Vir, de quo ſuperiùs memini, Emanuel Pimentel, in ſuis obſervationibus per umbram rectam Gnomonis. Ac proinde nec mirum, ſi qua intercedat diverſitas, cùm non una ſit inſtrumentorum conditio. Sed hæc hactenus de Ulyſſiponis geographica latitudine.

Obſervationes Aſtronomicæ Telone Martio *habitæ à R. P.* Antonio Laval, *Soc.* JESU, *Hydrographo Regio, &* Ulyſſiponem *miſſæ P.* Joanni Baptiſtæ Carbone *ejuſdem Soc.*

Emerſiones intimi Satellitis Jovis.

1725.		H.	′	″				H.	′	″	
Sept. 23.	Emerſio	6	56	42	T.V.	*Dec.* 17.	Emerſio	5	56	34	
Oct. 16.	Emerſio	7	15	17		*Dec.* 24.	Emerſio	7	49	18	dub.
Nov. 8.	Emerſio	7	31	33		1726.					
Nov. 15.	Emerſio	9	26	52		*Jan.* 9.	Emerſio	6	2	3	dub.

Die

Die 18, Januarii 1726, Martis *Occultatio à* Luna *Temp. Ver. Hor.* 7. *Min.* 23, *Veſpere.* Non ſatis certa.

Emerſio Martis *H.* 8, *Min.* 21, *Sec.* 34, certa.

Altitudines Meridianæ apparentes Veneris.

1725.	Gr.	′	″
Mart. 20.	36	34	30
April. 21.	51	43	0
Maii 8.	59	35	0
Sept. 8.	44	30	30
21.	37	57	0
24.	36	26	30
Octob. 18.	26	28	45
Nov. 8.	21	50	0
Dec. 7.	23	59	30
Dec. 21.	28	21	0
Dec. 24.	29	30	0
1726.			
Jan. 9.	36	29	0
19.	41	19	0
31.	47	14	0
Febr. 3.	48	40	30

Ex permultis accuratiſſimis obſervationibus *Telonis Martii* latitudo inventa eſt 43° 6′ 55″.

Die 20, Martii 1725.

	Gr.	′	″
Altitudo apparens Limbi ſuperioris Solis quadrante Aſtronomico trium pedum Pariſinorum obſervata *Telone Martio,*	47	6	10

Die 21, *ejuſdem Menſis.*

Altitudo apparens ejuſdem Limbi -----	47	30	10

Ex his obſervationibus inferri poſſet ingreſſus Solis in Æquinoctium Vernum. At non ſatis exactè, ut mihi videtur.

Differentia Meridianorum Ulyſſiponem inter, ac Telonem Martium ex pluribus obſervationibus eruta eſt. 1 h. 0′ 9″; ſeu, Gr. 15 2′ 15″, quibus Ulyſſipo occidentalior eſt Telone Martio.

III. *Singularia quædam in Cadavere calculoſo diſſecto reperta, ab* Abrahamo Vater, *M. D. Pr. Anat.* Wittemb. R.S.S.

JUvenis ſtudioſus per duos annos Dyſuria frequenti laborans, per illud tempus ultra quinquaginta calculos excrevit, quorum plerique ſine inſigni dolore inter mingendum ſeceſſerunt; quidam tamen eorum reliquis majores, piſorum majorum vel phaſeolorum magnitudinem habentes, in urethra ſubſtiterunt, ac manu chirurgi comminuti aut ſectione extracti fuerunt. Incidit hic ſenſim in Maraſmum totius corporis, ac urgente tuſſi ſicca & Aſthmate, accedente inſuper tumore pedum œdematoſo, tandem pridie ante mortem, lecto affixus, animam Creatori reddidit. Ad ſectionem corporis defuncti accedentes, aperto Thorace, pulmonem cum pericardio, diaphragmate & coſtis, hinc inde concretum, ac in dextro præprimis latere ſcirrhoſum invenimus, in utroque prætereà ventriculo cordis, Polypos inſignes, qui truncos vaſorum occupaverant,& proculdubiò Aſthmatis atque inſequentis ſuffocationis, mortiſque ſubitaneæ præcipua cauſa fuerunt.

In abdomine hepar & lien nullo manifeſto vitio laborabant; inteſtinum autem ileon ſugillatum erat, & colon in toto ſuo ambitu, à dextro inde latere, ubi hepati accumbit, una cum recto, ita conſtrictum & contractum apparebat, ut vix digiti craſſitiem æquaret, nullamque ferè cavitatem interiùs monſtraret.

Tandem vias urinarias, utpote officinam tot calculorum viſitantes, in renibus atque ureteribus nihil præternaturalis invenimus, in veſica verò tres calculos, pha-

phaſeolorum magnitudinem habentes, eos tamen non liberos, ſed membrana valida involutos, ac lateri anteriori prope ſphincterem adhærentes deprehendimus, quos in rei memoriam una cum eorum involucro aſſervo. Unde hæc membrana, calculos involvens originem trahat, explicatu difficillimum mihi videtur; quamobrem hac data occaſione, flatu per ureterem utrumque immiſſo, inquiſivi, an illa (*viz* membrana) cum ureteribus communicet; & utrum ſit ipſa membrana Veſicæ interior, à calculis in ureteribus, inter ſubſtantiam Veſicæ incedentibus, una cum uretere, vel hoc rupto, extenſa: nihil tamen detegere potui. Hos tamen calculos Dyſuriæ non tantùm continuæ, ſed etiam propter irritationem perpetuam, conſtrictionis præternaturalis inteſtini coli atque recti extitiſſe, rationi conveniens videtur.

IV. *A Letter from Mr.* Colin Mac Laurin, *Profeſſor of Mathematicks at* Edinburgh, *and* F.R.S. *to* Martin Folkes, *Eſq*; V. *Pr. R. S. concerning Æquations with impoſſible Roots.*

SIR,

I Wrote to you laſt Winter, that I had thought of a very eaſie and ſimple way of demonſtrating Sir *Iſaac Newton*'s Rule, by which it may be often diſcover'd when an Æquation has impoſſible Roots. This Method requiring nothing but the common Algebra, and being founded on ſome obvious Properties of Quantities demonſtrated in the following *Lemmata*, without having recourſe to the Conſideration of any Curve whatſoever, which does not ſeem ſo proper a Method in a Matter purely Algebraical, I hope it will not be unacceptable.

Lemma 1. The Sum of the Squares of two real Quantities is always greater than twice their Product. Thus a^2+b^2 is greater than $2ab$; becauſe the Exceſs a^2+b^2-2ab is eqaal to $\overline{a-b}^2$, and therefore is Poſitive; ſince the Square of any real Quantity, Negative or Poſitive, is always Poſitive.

Lemma 2. The Sum of the Squares of three real Quantities is always greater than the Sum of the Products, that can be made by multiplying any two of them. Thus $a^2+b^2+c^2$ is always greater than $ab+ac+bc$; for 'tis plain, that the Exceſs $a^2+b^2+c^2-ab-ac-bc$ =

$= \frac{2a^2+2b^2+2c^2-2ab-2ac-2bc}{2} = \frac{a^2-2ab+b^2+a^2-2ac+c^2+b^2-2bc+c^2}{2} = \frac{\overline{a-b}^2+\overline{a-c}^2+\overline{b-c}^2}{2}$ that is, half the Sum of the Squares of the Differences of the Quantities a, b, c: But ſince theſe Squares are Poſitive, it follows, that the Exceſs of $a^2+b^2+c^2$ above $ab+ac+bc$ is Poſitive, and that the Sum of the Squares of three Quantities muſt be greater than the Sum of the Products made by multiplying any two of them.

Lemma 3. The triple Sum of the Squares of four Quantities is greater than the double Sum of the Products, that can be made by multiplying any two of them; for $3a^2+3b^2+3c^2+3d^2-2ab-2ac-2ad-2bc-2bd-2cd = a^2-2ab+b^2+a^2-2ac+c^2+a^2-2ad+d^2+b^2-2bd+d^2+b^2-2bc+c^2+c^2-2cd+d^2 = \overline{a-d}^2+\overline{a-c}^2+\overline{a-d}^2+\overline{b-c}^2+\overline{b-d}^2+\overline{c-d}^2$, the Sum of the Squares of the Differences of the four Quantities a, b, c, d. Therefore $3a^2+3b^2+3c^2+3d^2$ is greater than $2ab+2ac+2ad+2bc+2bd+2cd$, the Exceſs being always Poſitive.

Lemma 4. Let the Number of the Quantities a b, c, d, e, &c. be m, the Sum of their Squares A, and the Sum of the Products made by multiplying any two of them B. Then ſhall $\frac{\overline{m-1}\times A}{2}$ be always greater than B.

For by adding together the Squares of the Differences $a-b$, $a-c$, $a-d$, $b-c$, $b-d$, $c-d$, &c. you add a^2 as often to it ſelf as there are Quantities more than a; the ſame is true of b^2, c^2, d^2, e^2, &c. But the Rectangles $-2ab$, $-2ac$, $-2ad$, $-2bc$, $-2bd$, &c. ariſe but once each. Therefore the Sum of all the Squares $\overline{a-b}^2$, $\overline{a-c}^2$, $\overline{b-c}^2$, $\overline{b-d}$ &c. $= \overline{m-1}\times a^2 + \overline{m-1}\times b^2 + \overline{m-1}\times c^2$, &c. $-2ab-2ac-2bc$, &c. $= \overline{m-1}\times A - 2B$. But $\overline{a-b}^2$

+

$+\overline{a-c}^2 + \overline{a-d}^2$, &c. is always a Positive Quantity; therefore $\overline{m-1} \times A - 2B$ is Positive, and consequently $\frac{\overline{m-1} \times A}{2}$ greater than B.

Cor. It appears from the Demonstration, that the Excess of $\overline{m-1} \times A$ above $2B$ is always equal to the Sum of the Squares of the Differences of the Quantities a, b, c, d, &c. and that when the Quantities a, b, c, d, &c. are all equal, then $\overline{m-1} \times A - 2B = o$, and with this restriction on the preceding *Lemmata* must be understood.

It is to be observed, that tho' we have supposed in these *Lemmata* the Quantities a, b, c, d, &c. Positive, they are *a fortiori* true of Negative Quantities, whose Squares are the same as if they were Positive, while the Sum of their Products is either the same, or less than it would be, were they all Positive.

PROP. I.

In a Quadratic Æquation that has its Roots real, the Square of the second Term must be always greater than the quadruple Product of the third and first Terms.

Let the Roots of the Quadratic Æquation be represented by $+a$ and $+b$; and if x be the unknown Quantity, then shall $x^2 - ax + ab = o$
$-bx$

Now since $a^2 + b^2$ is greater than $2ab$, by *Lemma* 1, therefore $a^2 + b^2 + 2ab$ is greater than $4ab$; therefore $\overline{a+b}^2 \times x^2$, the Square of the second Term, will be greater than $4ab \times x^2$ the Quadruple Product of the first and third Terms.

PROP.

PROP. II.

In any Cubic Æquation, all whoſe Roots are real, the Square of the ſecond Term is always greater than the triple Product of the firſt and third.

If the Cubic Æquation has all its Roots real, they may be repreſented with their Signs by a, b, c, and the Æquation will be expreſſed thus:

$$\begin{aligned} & y^3 - a y^2 + a b y - a b c = 0 \\ & - b y^2 + a c y \\ & - c y^2 + b c y \end{aligned}$$

But by *Lemma* 2, $a^2+b^2+c^2$ is always greater than $ab+ac+bc$; and conſequently adding $2ab+2ac+2bc$ to both ſides, $a^2+b^2+c^2+2ab+2ac+2bc$ $(=\overline{a+b+c}^2)$ will be greater than $3ab+3ac+3bc$; and therefore $\overline{a+b+c}^2 \times y^4$ muſt be greater than $\overline{3ab+3ac+3bc} \times y^4$, that is, the Square of the ſecond Term muſt be greater than the triple Product of the firſt and third Terms.

Cor. 1. In general, it appears from the Demonſtration, that the Square of the Sum of three real Quantities, $\overline{a+b+c}^2$ is always greater than the triple Sum of all the Products, that can be made by multiplying any two of them into one another.

Cor. 2. It follows from the Propoſition, that when the Square of the ſecond Term is not greater than the triple Product of the firſt and third Terms, the Roots of the Æquation cannot be all real; but two of them muſt be impoſſible: And this plainly coincides with one Part of Sir *Iſaac Newton*'s Rule for diſcovering when the Roots of Cubic Æquations are impoſſible.

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He desires you may write above the middle Terms of the Æquation the Fractions $\frac{1}{3}, \frac{1}{3}$ as in the Margin; and placing the Sign + under the first and last Term, he multiplies the Square of the second Term by the Fraction $\frac{1}{3}$ that is above it; and if the Product is greater than the Product of the adjacent Terms, he places + under the second Term; but if that Product is less, he places — under the second Term, and says, there are as many impossible Roots as changes in the Signs. Now by this Proposition, if $p^2 x^4$ is not greater than $3 q x^4$, or $\frac{1}{3} p^2 x^4$ greater than $q x^4$, the Roots cannot be all real. The same Supposition makes two Changes in the Signs, whatever Sign you place under the third Term, since the Signs under the first and last are both +; and therefore this Proposition demonstrates the first Part of Sir *Isaac Newton's* Rule, as far as it relates to Cubic Æquations.

$$x^3 + p x^{\frac{1}{3}} + q x^{\frac{1}{3}} + r = 0$$
$$+ \quad - \quad * \quad +$$

Cor. 3. If the second Term is wanting in a Cubic Æquation, and the third is Positive, two of the Roots of the Æquation must be impossible: For the Square of the second Term (equal to nothing in this Case) will be less than the triple Product of the adjacent Terms. But this will better appear from considering that, when the second Term vanishes in an Æquation, the Positive and Negative Roots are equal, and when added together, destroy each other. Suppose the Roots to be $+a$ and $-b$ $-c$; then in this Case $a = +b+c$, and the Coefficient of the third Term will be $-ab-ac+bc = -b^2-2bc-c^2+bc = -b^2-bc-c^2$, and consequently Negative. Or, if you suppose two Roots Positive and one Negative, let them be $-a+b+c$, then the Coefficient of the third Term will be still $-b^2-bc-c^2$. Therefore when the Roots are real,

the

the Coefficient of the third Term is negative; and if the Coefficient of the third Term is not affected with a negative Sign, it is a Proof that two of the Roots are Impoſſible.

PROP. III.

In any Cubic Æquation, all whoſe Roots are real, the Square of the third Term muſt be greater than the triple Product of the ſecond and fourth Terms.

In the ſame Cubic Æquation, whoſe Roots are a, b, c, the Square of the third Term is $\overline{ab+ac+bc}^2$, the Product of the ſecond and fourth Terms is $a^2bc + ab^2c + abc^2$, as is plain from the Inſpection of the Æquation; and it is obvious that $a^2bc+ab^2c+abc^2$ is the Sum of the Products of any two of the Terms ab, ac, bc; and therefore by *Corol.* 1 *Prop.* 2. the Square of the Sum of theſe Terms, that is, $\overline{ab+ac+bc}^2$ muſt be greater than $3a^2bc+3ab^2c+3ac^2b$. So that $\overline{ab+ac+bc}^2 \times y^2$ muſt be greater than $\overline{3a^2bc+3ab^2c+3ac^2b} \times y^2$; that is, the Square of the third Term muſt be greater than the triple Product of the ſecond and fourth Terms.

Cor. 1. It follows from the Demonſtration, that $\overline{ab+ac+bc}^2$ is always greater than $3abc \times \overline{a+b+c}$.

Cor. 2. If the Square of the third Term is found to be leſs than the triple Product of the ſecond and fourth Terms, then the Roots of the Æquation cannot be all real Quantities; and this concludes with the ſecond Part of Sir *Iſaac Newton*'s Rule for finding when the Roots of a Cubic Æquation are impoſſible: For this Caſe gives — to be placed under the third Term, and

$$x^3 + px^{\frac{1}{3}} + qx^{\frac{1}{3}} + r = 0$$
$$+ \quad * \quad - \quad +$$

consequently two Changes of the Signs, whatever Sign is placed under the second Term.

Schol. After the same manner, it may be demonstrated, that in a Cubic Æquation, whose Roots are all real, if the second Term is wanting, the Cube of the third Part of the third Term taken positively, is always greater than the Square of half the last Term. Suppose that the Roots of the Æquation are $+a, -b, -c$, or $-a, +b, +c$, and that $a = b + c$, then the second Term in the Æquation will be wanting, and the other Terms will be expressed thus:

$$y^3 * \begin{matrix} -b^2 \\ -bc \\ -c^2 \end{matrix} y \pm bc \times \overline{b + c}$$

The Square of $b - c$ is always positive, since b and c are real Quantities. Suppose it, (*viz.* $b^2 - 2bc + c^2$) equal to D, then $b^2 + bc + c^2 = D + 3bc$, and $\overline{b + c}^2 = D + 4bc$. Therefore $\frac{\overline{b^2 + bc + c^2}^3}{27} = \frac{D^3}{27} + \frac{D^2 bc}{3} + Db^2 c^2 + b^3 c^3$, and $b^2 c^2 \times \frac{\overline{b + c}^2}{4} = \frac{Db^2 c^2}{4} + b^3 c^3$. Now 'tis obvious that $\frac{D^3}{27} + \frac{D^2 bc}{3} + Db^2 c^2 + b^3 c^3$ is greater than $\frac{Db^2 c^2}{4} + b^3 c^3$, since D is positive, and bc also positive, b and c being Roots having the same Sign. Therefore the Cube of $\frac{1}{3}$ of the third Term having its Sign changed $\left(= \frac{\overline{b^2 + bc + c^2}^3}{27}\right)$ is always greater than the Square of half the last Term $\left(= b^2 c^2 \times \frac{\overline{b + c}^2}{4}\right)$. In the Cubic Æquation $x^3 * + qx + r = o$, if q be positive, or if it

be

be negative and $\frac{+}{27}q^3$ be less than $\frac{1}{4}r^2$, it appears that two Roots of the Æquation must be impossible, from this Corollary, and from *Cor.* 3. *Prop.* 2. taken together.

PROP. IV.

In a Biquadratic Æquation, all whose Roots are real Quantities, $\frac{3}{8}$ of the Square of the second Term is always greater than the Product of the first and third Terms; and $\frac{3}{8}$ of the Square of the fourth Term is always greater than the Product of the third and fifth Terms.

1. Let the Æquation be $x^4 - p\,x^4 + q\,x^2 - r\,x + s = o$; and since the Roots are supposed to be all real, let them be represented by a, b, c, d, then $p = a+b+c+d$, and $q = ab+ac+ad+bc+bd+cd$. But it is plain from *Lemma* 3, that $3a^2+3b^2+3c^2+3d^2$ is greater than $2ab+2ac+2ad+2bc+2bd+2cd$; and consequently by adding $6ab+6ac+6ad+6bc+6bd+6cd$ to both, we shall find that $3\times\overline{a+b+c+d}^2$ must be greater than $8ab+8ac+8ad+8bc+8bd+8cd$; that is, $3p^2$ greater than $8q$; and therefore $\frac{3}{8}p^2x^6$ greater than qx^6.

2. Since $r = abc+abd+acd+bcd$, and $s = abcd$; and since qs is equal to $a^2b^2cd+a^2c^2bd+a^2d^2bc+b^2c^2ad+b^2d^2ac+c^2d^2ab$, which are the Products can be made of any two of the Quantities abc, abd, acd, bcd, whose Sum is r multiplied by one another; it follows, that $3r^2$ is always greater than $8qs$: So that $\frac{3}{8}$ of either the Square of the second Term, or of the Square of the fourth Term, must al-

ways be greater than the Product of the Terms adjacent to them.

Cor. Multiply either the Square of the ſecond Term, or the Square of the fourth Term of a Biquadratic Æquation by $\frac{1}{x}$, and if the Product does not exceed the Product of the adjacent Terms, ſome of the Roots of that Æquation muſt be impoſſible.

PROP. V.

In an Æquation of any Dimenſion expreſſed by m, the Coefficients of the ſecond, third, laſt, laſt but one, and laſt but two Terms, being reſpectively A, B, E, D, C, if the Roots of the Æquation are all real, then ſhall $\overline{m-1} \times A^2$ always be greater than $2mB$, and $\overline{m-1} \times D^2$ greater than $2mCE$.

1. For ſuppoſing the Roots to be a, b, c, d, e &c. then by *Lemma* 4, ſhall $\overline{m-1} \times a^2 + \overline{m-1} \times b^2 + \overline{m-1} \times c^2$ &c. be greater than $2ab + 2ac + 2ad$, &c. and adding $\overline{2m-2} \times ab + \overline{2m-2} \times ac + \overline{2m-2} \times ad$, &c. to both, the Sum $\overline{m-1} \times a^2 + 2\overline{m-2} \times ab + \overline{m-1} \times b^2 + \&c. (= \overline{m-1} \times \overline{a+b+c, \&c.}^2)$ muſt be greater than $2mab + 2mac + 2mad$, &c. that is, $\overline{m-1} \times A^2$ muſt be greater than $2mB$.

2. In general, it follows from this Demonſtration, that the Square of the Sum of any Quantities whoſe Number is (m) multiplied by $m-1$, muſt be greater than the Sum of all the Products can be made by multiplying any two of them, multiplied by $2m$. But it is eaſie to ſee from the Geneſis of Æquations, that CE is the Sum of the Products can be made by multiplying any two of the Terms whoſe Sum is D: From which it follows, that $\overline{m-1} \times D^2$ muſt be always greater than $2mCE$.

To be continued.

V. *Observations upon the Dissection of an* Ostrich, *communicated in a Letter to the Publisher, by the Ingenious Mr.* George Warren, *Surgeon in* Cambridge.

SIR,

I Thought my self obliged to lay before you a few Observations I made in dissecting the Male-Ostrich which died at *Sturbridge*-Fair; tho' indeed, Dr. *Brown* has so well describ'd the Parts of that which he dissected [*Philos. Collect.* N° 5] that I think, there is not much to be added. I presume, an Ostrich is allow'd now not only to be a Bird, but a Water-Fowl, and of the Goose-kind: For tho' I believe it does not swim, yet it has Legs long enough to go into deep Waters, and a Neck long enough to dip to the bottom of any Water where it can wade, and a Beak fitly made for the Prey of Fish, as that of a Goose is. Dr. *Brown* affirms it has no *Epiglottis*; but in this Subject that Cartilage was plainly visible; and indeed, the *Rimula* appeared too open not to require one. The *Os Hyoïdes* is three Inches long from the Basis; the *Musculi Directores Asperæ Arteriæ* were very plain, large and strong; the Ring composed of three Cartilages at the Divarication of the *Aspera Arteria* very bold; the two Glands on the Carotid Arteries, as large as small Eggs. There was nothing particular in the Lungs or Heart, but what it has in common with other Birds. The two Stomachs, *viz* the Crop and Gizzard, were filled with half-digested Grass, in which were some Nails, some Stones of the bigness of Walnuts, and about fourteen or fifteen

fifteen Pieces of Silver and Copper-Money. The firſt Stomach, or Crop was exceedingly tender, and contain'd, cramm'd as it was, between three and four Quarts. The Glands on the top of the Crop were very large and numerous, in the Order deſcrib'd by Dr. *Brown*, and of the Bigneſs of little *Oculi Cancrorum*, and of a watery-brown Colour; which being ſo different from the Colour of the Stomach, that added to the pretty Order they are placed in, makes 'em very remarkable. The Crop lay within the *Thorax*, but ſo that the Gizzard lay higher. The Looſeneſs and Likeneſs to Flannel, of the inner Coat of the Gizzard, mentioned by Dr. *Brown*, was very remarkable in this Bird; but the Texture in the Muſcular Part of it did not ſeem proportionably ſtrong to that in other Birds, being broader, thinner, and more flaccid. The Guts, as near as I could meaſure them, were about twenty ſix Yards long. The two *Cæcums*, which are about thirty four Inches long each, and which have beautiful ſpiral Valves, were Appendages of the very Beginning of the *Colon*. The Teſticles lay as in other Fowls, very high, and leſs than Pidgeons Eggs, but longer. I found the Liver to have four Lobes, and thought I had met with a Gall-Bladder, but it appear'd at laſt to be only the Membrane of the Liver rais'd by ſome Accident from its inner Subſtance. The Gland under the Stomach, which Dr. *Brown* ſuppoſes to be the Spleen, and the *Pancreas* and Kidneys anſwer his Deſcription; and the Ureters were, as he ſays, firm, ſtrong, white, long, and opening into the *Rectum*. The Eye is ſaid to be exactly like the Human Eye; but is indeed, a perfect Gooſe-Eye for its Colour, and, I believe, for the reſt of its Parts, as they are well deſcribed by Mr. *Ranby*: It was flatter than the Human Eye, as, I believe, all Birds are; and it had that ſimple Look ſo peculiar to the Gooſe.

Goose. The Bony Circle describ'd by Mr. *Ranby*, this Bird has in common with other Fowls both of the Water and Land, with this difference only, that the Ring in Water-Fowls consists of fifteen, and in Land-Fowls but of fourteen Bones. They are so dispos'd, that one Bone lies over the Ends of two others, then three or four lie over one another, like the Scales of Fish; then one Bone lies under the Ends of two others; and then two or three more follow again like the Scales of Fish: But unless there be a *Lusus Naturæ*, I think Mr. *Ranby*'s *Icon* does not express it so very justly, as I believe it might be done. There was no *Musculus Suspensorius Oculi* in this Animal, nor do I believe it is to be found among Birds, and indeed there seems to be no reason for it.

The Crop was so stuff'd with Grass, or other Greens (proper Food for a Goose, or one of that Kind) that I don't think the Bird could have digested it off, if there had been no other reason for its Death. The Gizzard was not so stuff'd as the Crop, and what was therein, seem'd undigested. The Guts contain'd a thick deep-green Juice, even to the *Cloaca*. The Money both of Silver and Copper in the Gizzard was very remarkably worn away; the Edges, in particular, were made round, and the Bust and Reverse scarce perceptible in some Pieces, and quite obliterated in others. The *Ærugo* and *Sulci* in many of the Pieces would make one believe (that besides the Attrition) there may be a *Menstruum* in their Gizzards not unfit to dissolve Metals. Within an Inch of the End of the *Rectum* was the *Cloaca*, or Expansion of that great Gut, which was thinner than the other Part of the Gut, in proportion to its Expansion, and would hold above half a Pint. The End of the *Rectum* (from the *Cloaca*) open'd into a Cavity big enough to hold my two Fists; and for want of another

other Name, I will call it the *Receptaculum Penis*, becauſe therein the *Penis* was always lodged when flaccid. I know, that Part is called by Dr. *Brown*, *a kind of Præpuce*; but upon Diſſection, it appeared plain enough to me to be a very ſtrong Muſcle compos'd of circular Fibres, and to be deſigned for a Sphincter of that Part wherein the *Penis* was to be lodg'd, and to be a Sphincter of the *Rectum* too; round which I traced the ſame Muſcle above an Inch; and this being but one Muſcle, muſt be the reaſon that the *Penis* always came out ſome Inches when it muted, as they told me it did. The *Penis*, flaccid as it is, is five Inches and an half long from the Skin of that *Receptaculum*, and, as Dr. *Harvey* ſays, not unlike an Hart's Tongue. I don't find a Cartilage in it, as Dr. *Brown* ſuggeſts; but at its Origination it is ſo hard a Body, that, I believe, if the Bird had liv'd ſome Years, it poſſibly might have become Cartilaginous. There are two Bodies that are join'd to the *Crura Penis*, which I ſuſpect to be the *Veſiculæ ſeminales*; and the more, becauſe there are two Veſſels enter them, which I believe to be the *Vaſa Deferentia*; but of this I am not certain: For tho' I found *Semen* in the *Urethra*, I have not been able to trace a Paſſage from theſe ſuppoſed *Veſiculæ ſeminales*, or thoſe Veſſels, or any other Part into the *Urethra*. I call it *Urethra*, becauſe there is no other Term fix'd that I know of, tho' the Urine does not paſs that way; but, as in other Birds, is mix'd with the groſſer Excrements in the *Cloaca*. The *Urethra* then, is only a *Sulcus*, or Gutter, from one end of the *Penis* to the other; which *Sulcus*, as the *Penis* lies flaccid in the *Receptaculum*, as I venture to call it, lies on one ſide; but upon Erection, the *Penis* turns towards the Belly, and the *Sulcus* is then at the Top, and lies conveniently enough for Conveyance of the *Semen*. If thoſe two Bodies are not the *Veſiculæ ſeminales* (as I mention'd before) they muſt

muſt be Elongations of the *Crura Penis*; but I think, they are of much too looſe a Contexture to ſerve that purpoſe. Whether the *Vena Cava*, dividing into two Branches to go into the Kidneys, and uniting again when it comes out, is ſingular to this Bird, or is in common with Geeſe and other Water-Fowl, I don't yet know; but ſo it was in the Oſtrich. I will ſuppoſe it to be in common, till I have examin'd further, ſince I know, that the *Cæcums* of the Oſtrich, which are ſo much taken notice of, are no more than what it has in common with other Fowls; and that a Chicken has two, as large, and as long in proportion as the Oſtrich: But after all, I muſt ſay, I had too little time allow'd me by the Maſter of the Oſtrich, and the Oſtrich had too much Fat for me to make a more accurate Diſſection. Now I have mentioned the Fat, I muſt tell you, the *Omentum* upon the Stomachs and Guts was ſix Inches thick at the Top, and decreaſing gradually, was near two Inches thick at the Vent, and was divided into two Parts in the Middle from the Top to the Bottom. What I found, that I was ſure was in common with other Fowls, I have not taken much notice of, unleſs repreſented by others differently from what I found, or ſo as if peculiar to this Bird. I forgot to take notice, that the Baſis of the *Os Hyoïdes* is of this Figure and the round Part at the Top is lodg'd in a proper Cavity in the top of the Tongue. Partly under the Baſis of the *Os Hyoïdes*, lies a Cartilage in the Front and very beginning of the *Aſpera Arteria*, which is not unlike the *Thyroïdes*; but other Cartilages in that Part it has none, but what forms the *Rimula*. The firſt 28 Cartilages of the *Aſpera Arteria* are not annular, the reſt (being about 226) are entirely annular; but as ſoon as it divaricates to go into the Lungs, they are not ſo again.

Cambridge, Oct. 17, 1725. *I am*, &c. G. WARREN.

VI. *An Account of a Perſon kill'd by Lightning at* Worceſter, *communicated in a Letter to the Publiſher, by* R. Beard, M. D. R. S. S.

The following Account was given to a Gentleman at the time the Affair happened; but not knowing whether it deſerv'd your Conſideration, I deferr'd ſending it till you were pleas'd to deſire it.

Worceſter, June 11, 1724.

WE had laſt Night continu'd Lightning in the Eaſt from Eight o' clock to Twelve; the Weather for ſome time before having been very ſultry, the Wind at N. E. and the Barometer at ſettled Fair. This Morning the Mercury ſunk, and the Sky became more Cloudy and Temperate, except a few hot Gleams; at Two in the Afternoon, ſeveral fierce Showers fell, attended with Flaſhes of Lightning and Claps of Thunder, that ſtill approach'd nearer us: Between Two and Three, a Flaſh came ſo violently upon me, ſucceeded ſo very quick by a low, unuſual, dreadful Sound, that I immediately went to the door, fearing ſome Miſchief near. I was ſoon call'd to an Officer's Lady (aged about 18, and breeding) kill'd by it in the adjoyning Street. I found her yet warm, and that ſhe had ſurvived the Stroke for 6 or 7 Minutes. The Fire-Marks they ſhew'd me, were Streaks of a Copper-Colour, branched from the Left Shoulder all-over the *Thorax*, and interſperſed here and there with irregular Spots, which gave occaſion for that Conceit publiſh'd in our News, that

that *curious Plants were drawn on her Boſom, as with the fineſt Pencil.* This ſad Accident happen'd in a Parlour-Window next the Street, that could contain about two Perſons: The Lady, it ſeems, terrified with the repeated Lightning and Thunder, (it having formerly been fatal to her Brother) deſir'd an Officer to change Places with her, that ſhe might be near her Husband; but ſhe was no ſooner ſeated by his ſide, than ſhe inclined ſide-ways, and ſpoke ſome Words; after ſhe was carried to another Room, ſhe ſaid, ſhe was gone, and then, that ſhe was blind, and ask'd for Water. The Husband was thrown along, together with the fortunate Gentleman that had juſt reſign'd his Seat; and a large Looking-Glaſs was lifted off the Hooks. The Landlord's Daughter, at work near the Lady, perceiv'd ſuch an Impulſe on the ſide of her Head, that her Hearing was much impair'd, *and upon every Peal of Thunder ſince, ſhe is affected in like manner, tho' not ſo ſtrongly.* The Gentlemen complained, that they were ſtupified, forced down they did not know why, unleſs it were for want of Breath; and of Pains and Numbneſs in their Limbs. They had likewiſe on different Parts of their Bodies ſuch reddiſh Wheals as were ſeen on the Fair Lady's Breaſt: But theſe Symptoms vaniſhed the next Day. The other two Perſons at the further End of the Room were untouch'd; they were all ſenſible of a ſulphurous Smell. The Pane of Glaſs exactly behind the Lady's Waſt, was perforated by a round Hole of an Inch and half Diameter, as if done with a Diamond, or rather a Wind-Gun; but no where thereabouts could I diſcern the leaſt Traces of Fire, or Heat, nor on the Lady's Clothes (having no Stayes on) the Signs of any Violence. On a more nice Examination of the Body, in the Preſence of the Friends, that Evening, I diſcovered on the left Loin, taking in

part of the Spine of the *Os Ilium*, which was ſomewhat ſwoln, a deep Contuſion of the ſame Dimenſion with the Breach in the Glaſs: The Skin was neither indurated, nor pierc'd: The Blood in the Capillaries all round, but chiefly up the Back, ſettled, the Colour of which was eaſily diſtinguiſhed from that of the Streaks, and the circular Impreſſion.

The *Phænomenon*, that cauſed this Misfortune, roſe from the N. E. firſt ſlid off the Gabel-Beam, and the Bricks on the back part of the next Houſe, fill'd a little Court with Flame and Smoke, then turned a Leaden Spout contrary to its former Direction, mounted over the Roof, and, cracking a Stack of Chimneys, dropt down at the Window where the Husband and Wife ſate. Some credible People that ſaw it, to their great Terrour, aſſure me, that it was a Ball of Fire, and that it burſt with the loudeſt Report they ever heard, and then with a hiſſing Noiſe, paſs'd about a Yard from the Ground through an adjacent Street, and rowl'd off to to the S.W. Some Workmen there, and on a neighbouring Hill, obſerved the ſame.

In my Opinion, the Mortality of this Blow may be accounted for from the known Effects of impriſon'd Air only, when ſet at liberty, as the Appearances on the Skin may, from other active Particles hurried along with it at the time of the Exploſion. The *Impetus* being firſt received on the Parts deſcrib'd, occaſioned her Death to be leſs ſudden than uſual in ſuch Caſes.

Worceſter, June 4, 1726.

We have had more Lightning and Thunder the Week paſt, than ever has been known in that ſpace of time: And what was more extraordinary, the Continuance of it for 9 or 10 Hours together, with little or no Intermiſſion, and at ſuch a height above us. So far has this been

been from doing any great Damage near us, that in the Opinion of the Country-Farmers, it had very good Effects, eſpecially at the beginning during the Heat: For the little Inſects, that in ſome Places threaten'd the Deſtruction of the Hops and ſome other Plants, fell off like Bees by the Steam of a lighted Match. I know not whether this has been obſerv'd to you before; but I ſaw ſuch remarkable Inſtances of it in certain Situations, that I could not help giving you this ſhort Account of it.

I am, &c.

R. Beard.

VII. *An*

VII. *An Account of the* Strata *met with in digging for Marle, and of* Horns *found under Ground in* Ireland; *in a Letter from Mr.* James Kelly *to the Right Honourable* Edward Southwell, *Esq;*

Honour'd Sir, *Downpatrick, Dec.* 22, 1725.

SINCE I know you to be a Gentleman very curious in ſearching after Nature, I thought it would not be unacceptable, to give you an account of theſe Appearances, that we meet with in ſearching for Marle, now in ſo plentiful a manner found on your Eſtate in this Country.

Our Marle is found no where but in the Bottoms of low Boggs, where we ſearch for it with Augres, and find it at the depth of ſeven, eight, or nine Foot: This in many Places occaſions great Expence in draining off the Water. When we think to dig for it, we chuſe out ſix able Labourers and a Supernumerary; then we cut up a Hole twelve Foot ſquare; becauſe we judge that this Number of Men will manage that Pit in one Day, *viz.* two Men to dig, two Men to throw it up, and two Men to throw it by. The Supernumerary ſupplies Defects in every part, as will be found neceſſary. For the firſt three Foot, we meet with a fuzzy ſort of Earth, that we call *Moſs*, proper to make Turf for Fuel; then we find a *Stratum* of Gravel about half a Foot; under which, for about three Foot more, we find a more kindly Moſs, that would make a more excellent Fuel: This is altogether mixt with Timber, but ſo rotten, that the Spade cuts it as eaſily as it doth the Earth: Under this,

for

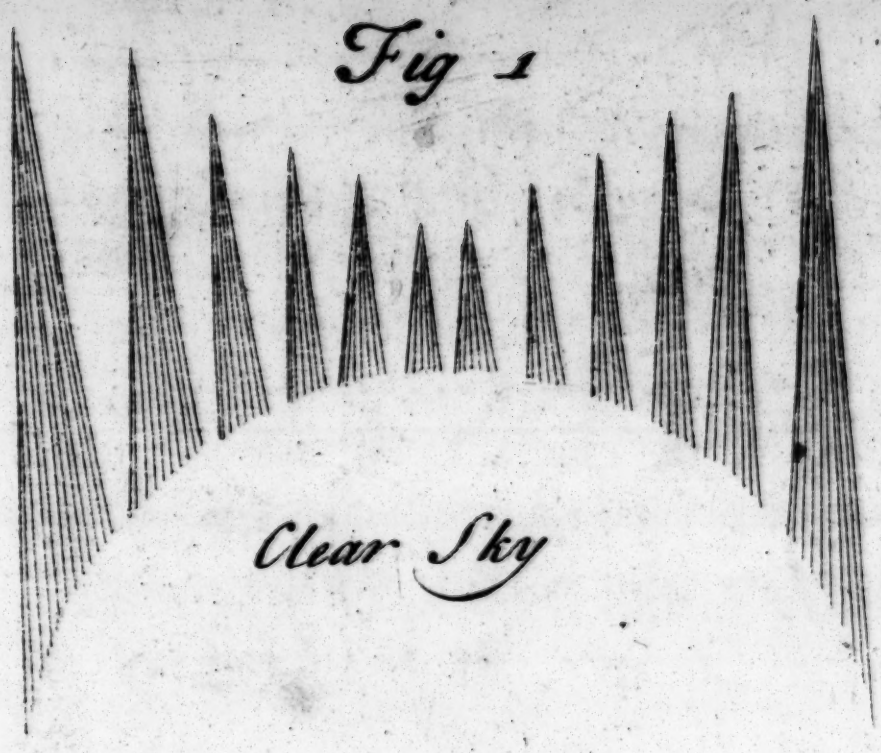
for the depth of three Inches, we find Leaves, for the moſt part Oaken, that appear fair to the Eye, but will not bear a Touch. This *Stratum* we find ſometimes interrupted with Heaps of Seed, that ſeem to be Broom or Furze-Seed: Nay, in one Place I ſaw, what appeared to me to be Gooſeberries and Currants: In other Places in the ſame *Stratum* we find Sea-weed, and other things as odd to be at that depth: Under this appears a *Stratum* of blue Clay, of half a Foot thick, fully mixt with Shells; this we look upon to be good Marle, and throw it up as ſuch: Then appears the right Marle, commonly 2, 3, or 4 Foot deep, and in ſome Places much deeper, which looks like buried Lime, or the Lime that Tanners throw out of their Lime-Pits, only that it is full-mixt with Shells: Theſe are ſmall Perriwinkles, ſuch as the *Scots* call *Freſh Water-Wilks*; tho' there are among them abundance of round red Perriwinkles, ſuch as I have often ſeen thrown out on the Sea-Shore. Among this Marle, and often at the bottom of it, we find very great Horns, which we, for want of another Name, call *Elk-Horns*: Where they joyn the Head, they are thick and round; and at that Joyning there grows out a Branch of about a Foot long, that ſeems to have hung juſt over the Beaſt's Eyes: It grows round above this for about a Foot and ſome odds; then ſpreads broad, which ends in Branches, long and round, turning with a ſmall Bend. The Labourers are commonly ſo buſie, that they rarely bring them up whole; yet I have one pretty well, of which I ſend you an *Icon*, *V. Fig.* done as well as I could, but not ſo nice as I could with. We have alſo found Shanks and other Bones of theſe Beaſts in the ſame Place.

I am, &c.

James Kelly.

F I N I S.

Fig 1



Clear Sky

Fig. 2

PHILOSOPHICAL TRANSACTIONS

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V. *A*

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151 ALchimilla vulgaris, C.B. 319.
152 — — Alpina, pubeſcens, minor, H. Reg. Par.
153 Aſter Americanus Belvidere foliis, floribus ex cœruleo albicantibus, ſpicis prælongis. Pluk. Phyt. Tab. 78. f. 5.
154 — Virginianus, anguſtifolius, ſerotinus, parvo albente flore, Park. Th. 132.
155 — —, pyramidatus, Hyſſopi foliis, aſperis, calycis ſquamulis foliaceis.
156 — maritimus flavus, Crithmum chryſanthemum dictus, Raii Hiſt. 268.
157 Brunella Caroliniana, magno flore, dilutè cœruleo; internodiis prælongis.
158 — Novæ Angliæ, major; foliis longiùs mucronatis.
159 Caucalis Hiſpanica, Cam. Hort. 37. Fig. xi.
160 Corchorus Americana, prælongis foliis, capſula ſtriata, ſubrotunda, brevi, Pluk. Phyt. Tab. 255. f. 4.
161 Corona Solis Caroliniana, parvis floribus, folio trinervi amplo, aſpero, pediculo alato.
162 — — Rapunculi radice, T. Inſt. 490.
163 Dictamnus Creticus, C.B. 222.
164 Dorycnium Monſpelienſium, Lob. Icon. 2. 51.

165 Elichryſum ſylveſtre, latifolium, capitulis conglobatis, C.B. 264.

166 Eupatorium Senecionis facie, folio Lamii, Par. Bat. Ic. 161.

167 Fœnum Græcum, quod Melilotus corniculis reflexis major, C.B. 331.

168 Fumaria minor, tenuifolia, cauliculis procumbentibus & caducis, C.B. 143.

169 Geniſta Hiſpanica humilis, floribus congeſtis villoſis. Sparto primo affinis, C.B. 396.

170 Geranium Cicutæ folio, erectum, Romanum, Bocc. Muſ. p. 2, 93.

171 — Columbinum erectum, Carolinianum, floribus purpuraſcentibus.

172 Hermannia fruteſcens, foliis oblongis, ad apices ſolummodò ſerratis.

173 Jacobæa Africana, fruteſcens, flore amplo purpureo elegantiſſimo, Senecionis folio, Volk. Flor. Norimb.

174 Jacobæa Sicula, Chryſanthemi facie, Bocc. rar. p. 66.

175 Lychnidea Virginiana, Holoſtei ampliore folio, floribus umbellatis, purpureis.

176 Marrubium album, foliis amplis, ferè circinatis.

177 Myoſotis Hiſpanica ſegetum, T. Inſt. 245.

178 — Orientalis perfoliata, Lychnidis folio, T. Cor. 18.

179 Pentaphylloides erectum, J.B. 2. 398.

180 — Canadenſe, folio Agrimoniæ. Sarracen. D. Vaillant.

181 — Fragariæ folio, Bot. Monſp. App. 309.

182 — — — & crenis obtuſioribus.

183 Quinquefolium rectum luteum, C.B. 325.

184 — quod Pentaphyllum, ſeu potiùs Heptaphyllum rectum, caule rubro, hirſutius, H. Cath. Boerh. Ind. 2 40.

185

185 — minus, flore pallidè luteo, T. Inſt. 297.
186 — folio argenteo, C. B. 325.
187 Phlomis fruticoſa, humilis, latifolia, candidiſſima; floribus luteis.
188 — Orientalis, lutea, herbacea, latifolia, verticillata.
189 Sclarea rugoſo, verrucoſo & laciniato folio, T. Inſt. 180.
190 Senecio Ægyptius, folio Matricariæ, Boerh. Ind. I. p. 40.
191 Sideritis arvenſis, latifolia, glabra, C.B. 233.
192 — Vulgaris hirſuta erecta, C.B. Ib.
193 Serratula annua, ſeminibus nitidiſſimis ad baſin compreſſis. Senecio carduus Apulus, Col. p. 1, 324.
194 — —, — —, majoribus & minimè compreſſis. Crupina Belgarum vulgò.
195 Teucrium Bæticum, Cluſ. Hiſp. 229.
196 — —, humilius & ramoſius, folio ſubrotundo.
197 Thlaſpi arvenſe, Vaccariæ incano folio, majus, C.B. 206.
198 —, Vaccariæ incano folio, minus, C.B. Ib.
199 Virga aurea, Canadenſis, Aſteriſci folio, Par. Bat. 244.
200 — —, montana, Scrophulariæ folio, Icon. Robert. Pluk. Phyt. Tab. 235. f. 3.

II. *An Account of an* Aurora Borealis *ſeen in* Ireland *in* September 1725; *in a Letter from* Arthur Dobbs, *Eſq; to his Brother the Reverend Mr.* Richard Dobbs, *Fellow of* Trinity-College, Dublin.

Caſtle Dobbs, Sept. 29, 1725.

I Here ſend you an Account of an *Aurora Borealis*, which I ſaw here the 24*th*, 25*th*, and 26*th* Inſtant, with Reflections upon it; which, if compar'd with others, or the ſame view'd at other Places, may in ſome meaſure contribute to clear up the nature of it.

I obſerv'd, that the Theatre of Light forming an irregular variable Curve, was, as at moſt times formerly, from E. N. E. to W. N. W. the Horizon and whole Hemiſphere ſerene, little or no Wind, what there was, ſeem'd Northerly. The ſeeming Dawn, or Stage of Light generally continu'd in an irregular Curve; the one Point in the two firſt Nights whilſt I obſerv'd it, began near the Horizon, near N.N.E. the other Point was at W.N.W. the Height of the Arch not exceeding 20 Degrees, in which there ſeem'd to be a continual Dawn: Under that Field of Light ſeem'd to be a dark Cloud, which, however, was a clear Sky, not filled with that luminous Vapour; becauſe all the Stars appear'd diſtinctly and twinkling thro' it. Whenever that Light roſe about 10 Degrees higher, to about 30 Degrees, then Flaſhes, or Coruſcations follow'd alternately, and ſeemed to be Pillars or Beams of Light, which follow'd or ſucceeded one another, and by that means

means ſeem'd to move and change with one another, by the Succeſſion of Light and Darkneſs, according to the Flaſhes. When the lighted Vapour roſe higher to about 40 or 45 Degrees, then the Appearance alter'd; and inſtead of Beams, or Pillars of Light, as when lower, there were Flaſhes like thoſe attending Exploſions, wherein faint Colours of Red, Green and Yellow appeared, but not very vivid; and upon each Exploſion it would ſpread upwards towards the Zenith, in the Appearance of thin enlighten'd Clouds, and immediately diſappear. On the 26*th*, about 9 at Night, one of theſe irregular Arches of Light had got up to the Zenith, the lower Points being near E. N. E and W. S. W. I then ſaw it for a conſiderable time, at leaſt a quarter of an hour, and it had been there for ſome time before I ſaw it. I could diſtinctly obſerve all the different Appearances, according to its Altitude in the Hemiſphere, *viz.* the lower part (being within 12 or 14 Degrees, as near as I could compute) was a conſtant fixed Light, equal to the Light of the Edge of a white Cloud in the Day-time, when the Sun ſhines on it. As it roſe higher, I could obſerve it ſomewhat weaker, and could perceive the Motion of the Pillars or Beams of Light after each Flaſh, which ſeemed by that means to move. Somewhat higher again, at about 40 Degrees, the Flaſhes were like Exploſions of great Guns, with the faint Colours obſerved as before; but the Coruſcations or Flaſhes from thence to the Zenith, expanded at every Flaſh, like a broad, thin, white Cloud, of which ſome faint View could be ſeen after each Exploſion for ſome time: And after all the Exploſions were over, there remain'd a thin duskiſh Vapour in and near the Zenith, and all along the Arch from Eaſt to Weſt, from 14 to 20 Degrees broad, which undulated and mov'd like a ſtormy Sea, the Motion coming

from

from the S.S.E. and ſo leſſen'd till it appear'd no brighter than the Milky Way, but more like a very thin Cloud or Miſt, thro' which I could perceive the Stars. At the ſame time I ſaw another thin Cloud, having the ſame Appearance, Arch-ways, to the Southward, at about the Height of 40 Degrees, which I ſuppoſe had been another, which had been over, and had moved thither from the Northward before I went out: And during the whole time there were leſſer Lights towards the North, but diſperſed here and there, and not forming any large Body of Light. During the whole time, the Hemiſphere was clear, except a few very ſmall Clouds near the Horizon; and when any moved into the enlighten'd Arch, they broke the Connexion, ſo that the Light was above them: At the ſame time it froze hard each Night.

From theſe Obſervations, I ſuppoſe that the *Aurora Borealis* is a thin Nitro-ſulphureous Vapour rais'd in our Atmoſphere conſiderably higher than the Clouds, which is diſcontinued in ſeveral Places by the interſpers'd Air, and which by Preſſure and Motion is kindled; and perhaps the Exploſion of one may by its Shock and Motion contribute to kindle the next; by which means they go off one after another, till the whole Vapour within their Influence is diſcharged, and then the Light diſappears, and the thin Smoak appears, and undulates, according to the Motion in that part of the Atmoſphere. And hence I think, moſt of the Appearances may be ſolv'd: For 1*ſt*, As to the continu'd Light near the Horizon, they being at a great diſtance from us, and nearly in a Line, all theſe Exploſions may ſeem as a continued Light: When theſe approach nearer to us, and by conſequence appear higher in our Hemiſphere, we obſerve the Motion in each Flaſh, and ſtill ſeeing them laterally, yet ſomewhat breaking the

Con-

Continuity of the Light; they (by the Reflection of the Vapour floating in the Atmoſphere, and being not reflected, where the Air betwixt them is free of thoſe Vapours) may appear as Pillars: And as the Flaſh below and beyond them moves (as it kindles and expands) ſo they ſeem to move, and perhaps are ſhock'd at the ſame time by the Motion; but afterwards, when they are nearer, and raiſed to the Altitude of 40 Degrees, we get ſomewhat under them, and ſee the Expanſion of the Exploſion, which appearing ſomewhat globular, gives the faint Colours obſerved above, the Light not being intenſe enough to make them vivid; and afterwards when they riſe to, or near the Zenith, they are nigheſt to us, and then expand very wide at each Flaſh, like little Clouds: And I think, the great Objection of their appearing in the Northern Part of the Hemiſphere, and ſeldom or never in the Southern, is in ſome meaſure anſwer'd by the Appearance on the 26*th*; ſince at leaſt half of the Arch was in the Southern Part of the Hemiſphere; and perhaps the Reaſon why the Light is not ſeen near the Horizon, in the Southern Part of the Hemiſphere, may be this, that in clear ſerene Weather, the Wind being generally near the North, Objects from thence are much more diſtinctly view'd, and at a greater diſtance than from the South; and 'tis generally known, that Lands at a great diſtance are moſt diſtinctly ſeen, when the Winds blow from them.

And perhaps a cold Northerly freezing Air may be needful to kindle the Vapours, when a contrary Motion above (higher in the Atmoſphere) may carry the Sulphureous Vapour, which falling down from the Nitrous Vapour may be kindled. Which, I ſuppoſe, form the Undulations of the Smoak after the Ex-

Explosion, which seemed, as above, like a stormy Sea moving from the S.S.E. Note, The Barometer was low for some Days before and after it.

I am, &c.

Arthur Dobbs.

III. *An Account of the* Aurora Borealis *that appear'd* Oct. 8. 1726. *In a Letter to the Publisher from the Reverend Dr.* Langwith, *Rector of* Petworth *in* Sussex.

THE Northern Lights have been so common in all Places of late Years, that tho' I have often seen them here, I did not think it worth while to write to you about them; but those that appear'd on *Saturday* the 8*th* of the last Month, were too remarkable to be pass'd over in Silence. They began about Sun-set; but I heard nothing of them till between 7 and 8. When I went out, I observ'd a Stream of Light almost due West, which was about seven or eight Degrees broad, and extended it self upwards about 35 or 40 Degrees. I had not a free Prospect of the Western Horizon, and so cannot tell what its Appearance was below. It was not perpendicular to the Horizon, but inclined a few Degrees towards the South. This Stream was of a dusky Red towards the North, but pale on the other side, and seemed to have a faint Mixture of the Prismatick Colours in it.

At the ſame time there appear'd a pale luminous Arch, whoſe Middle was nearly N. W. by N. The Altitude of its inner Edge was about 18 or 20 Degrees. This Edge was very diſtinct and regular all above, but a little confus'd towards the Horizon, where it extended it ſelf beyond the North-Point: How it terminated to the Weſt, I cannot inform you. From the upper Side of this Arch, which was waving, and ill defin'd, there ſhot up continually ſuch Streams of Light as have often been ſeen and deſcrib'd, ſince the Great Meteor of *March* the *6th*, $171\frac{5}{6}$. The Sky under this Arch look'd exceeding dark, but was in reality clear; for we cou'd ſee the ſmalleſt Stars in it.

Nearly N. E. there was another Stream of pale-colour'd Light, which was about 7 or 8 Degrees diſtant from the Horizon, and was about as many in Breadth: It's Height was various, and ill defin'd. Towards the bottom of it, was an irregular black Cloud, which in ſome parts was near a Degree in breadth, in others hardly half ſo much: This Cloud was almoſt parallel to the Horizon. The Stream mov'd with a ſlow regular Motion towards the Eaſt.

In the S. E. was another Arch, like that in the N. W. by N. but not quite ſo high, or of ſo great an Extent. Between this Arch and the North-Eaſterly Stream the Sky was of an odd pale colour'd Light, with a mixture of Red in it.

From the South towards the Weſt were gloomy irregular Clouds, which now and then ſent out Flaſhes of Light.

About 8, the North-Eaſterly Stream ſuddenly expanded it ſelf every way: All its Parts began to be in a violent Commotion, and its Brightneſs increas'd to ſuch a degree, that I remember nothing like it in the former great Meteor of this Kind. All above it was of a

bright flame-colour; but below, it was edg'd with the Priſmatick Colours, which were full as ſtrong as I have ever ſeen them in the brighteſt Rainbow: They were not indeed ſo diſtinct; for, tho' I obſerv'd them as exactly as the ſtrange variety of their Motion wou'd permit, I cou'd only diſtinguiſh the Red, the Yellow, and a dusky bluiſh-Green.

This ſurprizing Sight did not laſt above a minute or two; but when the Colours vaniſh'd here, they began to appear in the North-Weſterly Arch, which was now become a portion of a larger Circle than before, and was not elevated ſo high above the Horizon. The Colours extended themſelves from the North towards the Weſt for about 15 or 20 Degrees; and tho' they were not ſo bright as in the other Place, yet they were more ſteady, and ſo as eaſily obſerv'd. Their Order was the ſame as before, the Red lowermoſt, and ſo on: Their Duration much longer.

In the mean time the Streaming Lights began to appear in all parts of the Heavens, and to form a *Corona* and Canopy, which were in all reſpects like thoſe of the great Meteor of 171$\frac{5}{6}$. Inſtead therefore of troubling you with a long detail of the Particulars of theſe, I ſhall refer you to the curious deſcriptions of the other by the Aſtronomer-Royal, and my late worthy Friend Mr. *Cotes*. I ſhall only take notice that the Colours of the *Corona* were neither ſo ſtrong nor ſo laſting as thoſe before deſcrib'd, and that the Top of the Canopy was ſometimes over-ſpread with a deep ſullen Red.

The Streams continu'd their direction upward towards a point of concourſe for a long time after, and form'd by fits imperfect Circles of pale Light about it: This Point, however, was not fix'd; for at firſt it ſeem'd to be in, or very near, our Zenith; but when I ob-

obſerv'd it ſome time after, it lay between the Stars in *Andromeda*'s Right-hand, and thoſe at the end of her Chain. The ſame Obſervation was made by a Curious Gentleman of this place, who alſo inform'd me that there was another luminous Arch which paſs quite thro' the Pole-Star: It's continuance was ſhort, and I had not the good fortune to ſee it my ſelf.

Theſe Appearances held on in ſome degree till about 11, when the Air began to grow miſty, and ſo put an end to any farther Obſervations.

I cannot ſend you the exact Point of the Wind: It was ſo calm below, that I cou'd not be certain which way it ſtood; but ſome that were making their Obſervations from a high open Place, aſſur'd me, that it was North-weſterly, as it was in the Afternoon before, and the Morning after.

The Mercury was up at 30: The Weather mild and temperate.

I am afraid this Letter is already tedious, and yet I ſhall venture to lengthen it with the following Obſervations.

1. That It plainly appears from the Poſition of the Arches, that they cou'd not owe their Figures to the Sun: They ſeem to have been partly Optical, and partly to have depended upon the different Heights of the luminous Vapours; but for want of ſufficient *Data*, it will be no eaſy matter to determine how far each of theſe Cauſes concurr'd.

2. The Priſmatick Colours, wherever they appear'd, ſeem to have been caus'd by the Sun.

3. None of the Streams, as far as I could obſerve, proceeded directly from the Horizon. They were neareſt it towards the North, where there were ſome weak irregular Lights in the confus'd parts of the Arch before deſcrib'd.

4. I find by ſome of my Papers that during the Meteor of 171$\frac{5}{7}$. the Mercury ſtood at 30.2; ſo that the two Meteors agree, as in many other particulars, ſo in the following, *viz.* That the Air was calm, The Wind North-weſterly and the Mercury high.

I ſhall only add, that luminous Vapours in the Air are much more common than they are generally taken to be; for the Nights are very often lighter when the Sky is over-caſt, than in the brighteſt Star-light, tho' the *Crepuſculum* be quite gone off and there be no Moon.

I am, &c.

Petworth, Nov. 16. 1726.

B. Langwith.

IV. *The ſame Appearance deſcrib'd in a Letter to the Publiſher, by Dr.* Huxham, *Phyſician at* Plymouth.

THO' the Phænomenon, call'd *Aurora Borealis*, hath been frequently ſeen of late Years, even in this Part of the World; yet, I think, we have hitherto had but very imperfect Conjectures on the Cauſes of its Production. As I conceive a faithful Hiſtory of theſe Appearances will very much conduce to the Diſcovery of their true Cauſes, I do my ſelf the Honour to give you the following Account of that, which appeared the 8*th* inſtant, as we had it here at *Plymouth*. About half an hour paſt Six, perceiving *Jupiter* ſhone very bright, I was applying my Teleſcope to obſerve him, when on a ſudden ſeveral luminous Streaks appear'd about 10 Degrees above the Horizon in the N.E. and the Hemiſphere ſeem'd much enlighten'd. Imagining this to be the beginning of a *Lumen Boreale*, I caſt my Eye carefully along the Northern Horizon from E. to W. and very nearly in the W. Point I perceived, as it were, a vaſt red fiery colour'd Obelisk ſhot it ſelf up to the Height of 30 or 40 Degrees, which ſeem'd perpendicular to the Horizon, and its Baſe ſeem'd to inſiſt on it. Its Point almoſt touch'd the bright Star in the Northern Crown; a ſmaller Column or two ſtood near it, of the ſame Colour and Shape. The Light, in the mean time, to the Eaſtward increas'd conſiderably, and became more vivid; as when the Moon is behind a very bright

Cloud

Cloud. It alſo form'd it ſelf into Columns, which were projected to no great Height, and wou'd ſoon vaniſh, then ſoon return, and appear'd not only in the N. E. but alſo more Northerly.

In about a quarter of an hour from my firſt Obſervation, as from an Arch, or black Baſis (I know not better how to expreſs it) extended all over the Northern Horizon, which ſeem'd to interſect it nearly in the W. and E. N. E. Points, aroſe abundance of pyramidal Columns of Light on all parts of it; now here, now there, of unequal Bigneſs, Height and Luſtre; now ſuddenly gleaming forth, then as ſuddenly diſappearing; but thoſe Columns, that were to the Eaſtward of the N. were more bright and lucid than thoſe to the Weſtward, which were of a more fiery, rutilant Colour. The great Column in the Weſt ſtill remain'd in the ſame Poſition, Height and Shape; as I obſerv'd, by applying my Eye to a Wall very near E. and W.

Between the Arch and the Horizon, appear'd as it had been a black, dusky Fog, from whence the Streams of Light ſeem'd every where darted forth: Yet however black this appear'd, we could diſcern the Stars very clearly thro' it. This Arch at its firſt Appearance ſeem'd not to be above 15 or 20 Degrees (at its higheſt part) above the Horizon; but it continually grew higher, and from all parts of it Cones of Light were every Moment ſhot up, which all ſeem'd to tend to a Point near the Zenith (as the Vertical Circles or Arches on a Globe tend to its Poles) tho' as yet none reach'd it by ſeveral Degrees.

After 7 a clock the Columns to the Weſtward appear'd bright and vivid as thoſe in the E. except thoſe very near the W. Tho' the Limb of the Arch wou'd ſeem ſometimes very regular and well defin'd; yet at other times it wou'd ſeem to ſink, now in the middle, then at one part, then at another; and ſometimes it would

riſe

riſe with the ſame Irregularity: But it was certain, that during the whole time of the *Phænomenon*, no Light, or flaſhes of Light did appear in the black *Area* included between the Arch and the Horizon; even when it was at its geeateſt Height, which was about 10 or 12 Minutes before 8, when I judg'd it to be at leaſt 40 Degrees above the Horizon. Then from all parts of the Arch, but firſt from the Northern or higheſt parts of it, were Rays, or lucid Columns of a ſurprizing Brightneſs and Luſtre, darted with incredible Velocity towards the *Vertex*, where the Cuſps of of the converging Columns ſeem'd nearly to center; and ſuddenly from every Quarter of the Heaven, bright, ſhining Streams of Light were ſhot towards the Zenith; which meeting about 6 or 8 Degrees to the Southward of it, form'd a ſmall Circle of two or three Degrees Diameter, whoſe Border was much more lucid than near its Centre: This Circle ſeem'd form'd between *Cauda Cygni* and the *Lizard*, then nearly upon the Meridian.

This beautiful *Spectrum* might be likened to the Star worn by the moſt Noble Order of the Garter, but the pyramidal *Radii* were here revers'd; and from the Southward the Rays or *Striæ* were not near ſo long as thoſe from the N. eſpecially thoſe from the due S. not reaching above 10 or 15 Degrees from the Centre, or Circle; whereas thoſe from the Eaſtern and Weſtern Quarters were very long, and reach'd almoſt down to the Horizon; eſpecially in the E. and W. Points. The *Radii* were in a continual and exceedingly ſwift Undulation, and appear'd of ſeveral very bright Colours, as white, red, green, yellow, for ſeveral Seconds; but the moſt permanent and predominant Colours were a fiery Red, with an Eye of Crimſon, and a bright Pearl Colour: The red Rays came moſtly from the Weſtward, and that Colour continued till the entire

tire Diſſipation of this radiant Canopy; the others dying away and leaving, as it had been a thin Smoak. The Vibrations of theſe radiant Columns were as ſwift as Flaſhes of Lightning, and inceſſant.

This ſurprizing Sight remain'd over us in its full Glory 3 or 4 Minutes, during which time the Rays were darted towards the Centre with prodigious ſwiftneſs, and did not ſeem to be ſhot from it. Sometimes they undulated like the Vapours ariſing from a Lime-Kiln, or from the Earth in very hot Weather, and all the upper part of the Hemiſphere ſeem'd to be, as it were, in a Convulſion.

In a ſhort Time this agreeable Scene vaniſhed, and was broken into ſmall flitting bright Clouds, which ſtill retain'd an undulating Motion; and Coruſcations wou'd every now and then break forth from them. At this time alſo I obſerv'd ſeveral Star-like Meteors fall, as is frequently obſerved in a bright ſerene Night.

Tho' our glorious *Cupola* diſappeared a very few Minutes after 8, yet very vivid Coruſcations were ſhot continually from the N. E. and N. W. Parts of the Heaven, which daſhing againſt one another near the Zenith, formed by their Colliſion momentany Arches of a Circle, nearly in the ſame Place and of the ſame Diameter with that above mentioned. None now proceeded from the South, and very rarely from the true North. The Coruſcations were always more red and fiery from the Weſtward than from the Eaſt, which were always more bright and luminous.

We were loſt in the Contemplation of the beautiful *Phænomenon* over our Head, and did not obſerve the Formation of a lucid Arch projected over all the Northeru Horizon, which ſeemed like the Arch

of

of a Rainbow, of one vivid, bright, yellowiſh Colour, and all under was, as it were, a very dark Cloud; tho' by viewing it with a Teleſcope, we could diſcern the minuteſt Stars: So that the Darkneſs only proceeded from the greatneſs of the Light juſt above it. From this, as from the former, aroſe very lucid, bright Columns on all parts of it. No Coruſcations appear'd under it. Its greateſt Height might be 20 or 30 Degrees. Some of the Columns ſeem'd to radiate even to the Zenith from this Arch.

About 9, this lucid Arch vaniſh'd inſenſibly, with moſt of the luminous *Radii*, or Columns; but, as it were, a very bright *Crepuſculum* ſtill remained all along the Northern Horizon, and ſeveral very bright Coruſcations would ſeem to be ſhot out of the pure Sky: This, more eſpecially, was obſerv'd in the N.E. About 11, I obſerved ſeveral Coruſcations ſtill breaking forth, and here and there a luminous Column; and ſeveral little bright Clouds ſeem'd irregularly ſcatter'd up and down the Hemiſphere, which ſtill retain'd their darting and quivering Motion. The Northern *Crepuſculum* remain'd as bright as ever, and ſo continued till paſt Two in the Morning.

There were but very few, and thoſe very ſmall Clouds to be obſerved during the whole time of this *Phænomenon*, and the Air was clear; yet all around, and between the lucid Columns, whenever, or in what part ſoever they appear'd, the Air would ſeem very thick and hazy; tho' immediately upon the diſappearing of thoſe gleaming Lights, the Sky would in the ſame Place appear very clear and ſerene. Nay, even thro' ſome of the very Columns we could plainly diſcern the Stars. Some Gentlemen thought they ſaw the bright Stars of the Swan thro' the *Corona* it ſelf.

As to the Weather preceding and following this *Phænomenon*, I need not be very particular, ſeeing you will ſoon have it in my Meteorologic Obſervations. The Morning was fair, tho' the Air was thick, and we had a great Dew: The Mercury was at 30 Inch. *Hawksby*'s Thermometer at 50, little, or no Wind. The Day was pleaſant and warm, and the Air grew much thinner. The Evening was ſerene; a very ſoft Breeze from N. and by W. About Five the next Morning, there were ſeveral Clouds form'd, and the Air was very thick and hazy, at Seven it was all Cloudy, and a few Drops fell.

Tho' I had before ſeen ſeveral faint Appearances of the *Aurora Borealis*; yet this, for Beauty, Luſtre and Duration, vaſtly exceeded any thing of that nature I had ever ſeen. Indeed, I ſaw not that of *March* 6*th*, 1716, being not then in *England*.

I am, &c.

Plymouth, Oct. 16, 1726.

J. Huxham.

V. *A Letter from Dr.* Hallet, *Phyſician at* Exeter, *to Dr.* Henry Pemberton, *F. R. S. on the ſame Subject.*

Exon. Octob. 12, 1716.

Octob. 3, 1726, at Nine in the Evening, I ſaw an *Aurora Borealis* (as 'tis commonly call'd,) in which there was nothing different from former Appearances, excepting that from the luminous Arch which appear'd in the North, were frequently ſhot off Parts of Arches towards the Zenith, which vaniſh'd there.

Oct. 8, Coming from the Country near Seven in the Evening, I obſerv'd a great Light in the Eaſt and Weſt, which ſoon extended it ſelf over our Heads, the North and South appearing dark at the ſame time. No Cloud was ſeen all that Day. A great Dew fell on a ſudden, with which the Streets were wet, as by a ſmall Rain. Half an Hour after Seven many Streams appeared in the North, which grew very bright, and darted frequently up to the Zenith. A Line drawn through the Baſes of them, made an Arch of a Circle, extending from the N. E. to the W. or S. W. But the Streams ſeemed to proceed from a clear Sky being diſtinct from one another at the Baſes, and not united by a luminous Arch or Cloud, as in the more uſual *Aurora's*. The Streams at the two Extremities of the Arch were brighter, wider and longer when they did not ſhoot, than thoſe on the Top of it. [See Fig. 1.] There was at the ſame time a luminous Arch extending it ſelf from the two Extremities of the above-mention'd *Aurora* through the South, at a conſiderable Altitude. About Eight o'

clock the Streams began to have an Horizontal Motion, propagating themſelves on both ſides towards the South; and in a Minute or two the whole Heaven was ſurrounded with them. Immediately they all extended themſelves up to a Point near the Zenith (I think, a little towards the Eaſt) where their Points were blended together in a confus'd manner. At the ſame time, every Stream, which before was white, appear'd ſtriped with all the Colours of the Rainbow; but the moſt prevailing Colour was a deep Red. It is impoſſible to expreſs the Beauty of this glorious *Umbrella*, which cover'd the whole Hemiſphere with its variegated Rays, the Colours of which ſucceeded one another in a regular Order. In the Center of theſe Rays was a confus'd Rolling, Agitation or Ebullition of a luminous Cloud, appearing like Smoak. In about ten Minutes (as I imagine) the Colours diſappear'd, and the Streams began to retire from the Zenith; preſently after which, they would frequently dart and ſhoot with great Celerity up to the ſame Point. This Darting and Flaſhing, together with a tremulous Motion from all ſides of the Horizon, I obſerved till 12. And I am informed by others, that it continued till Four in the Morning. The moſt conſiderable Rays came from the Eaſt and Weſt. Next Morning we had a Fogg.

The beſt Account which I can give of this *Phænomenon* is this: I imagine a thin Cloud compoſed of a Sulphureous Exhalation, hanging over us in the Air, at a conſiderable Height, parallel to the Horizon; the Length of it being very great from Eaſt to Weſt nearly; the Breadth of it (at firſt) not ſo great, but that we might ſee the Stars from under it to the North and South. The North Side of it, I ſuppoſe, firſt took

Fire,

Fire, and ſhot its Streams or Flames perpendicularly upwards, which being undiſturb'd by Winds, muſt appear ſtreight and pointed at the Top. The Baſes muſt make an Arch by the Rules of Perſpective: For, I think, an Horizontal Right Line, of a vaſt Length, and at a great diſtance from us (ſuch as I take the Northern Edge of this luminous Cloud to have been,) ſeen at a conſiderable height in the Air, muſt appear bent down into an Arch. On a ſudden the Fire propagated it ſelf to all parts of this Vapour. The whole Heaven muſt then appear cover'd with the ſame Streams, which tho' really parallel to one another, muſt appear bent into a *Cupola*. The ſhooting and darting of theſe Flames, and their Concourſe, together with a Smoak proceeding from them, muſt give that confus'd Cloud which was obſerved in the Center of this Canopy. The regular Diſpoſition of the Colours in every Stream, perhaps, you may account for. I think, the Red appear'd at the right hand in all of them. Somewhere in the *Philoſ. Tranſact.* I have met with an Obſervation of an *Aurora*, in which the Streams were colour'd only where they met, or croſs'd one another. Whether the Light of one Stream paſſing thro' another, may not be ſeparated into Colours by Refraction, I will not determine. You may think of a better Solution. If the Altitude of the Top of the Arch in the North had been taken here, and at the ſame time at another Place upon the ſame Meridian, whoſe diſtance is known, from thence I imagine, the Height of the Cloud (as I call it) might have been calculated.

I am, &c.

Wm. Hallett.

VI. *The ſame Appearance deſcrib'd by* John Hadley, *Eſq; F. R. S.*

THE *Royal Society* hath received ſo many and ſo full Accounts of the frequent Northern Lights, which of late Years have been ſeen in *Europe*, and particularly of that remarkable one of the 8*th* of *October* laſt, that it ſeems needleſs at preſent to give a minute Deſcription of the whole Appearance. I ſhall therefore only take notice of a few Particulars, which either have been omitted by others, or by ſome remarkable Circumſtances attending them, ſeem moſt likely to be of uſe to thoſe who employ their Thoughts in attempting to diſcover the Nature and Cauſes of theſe *Phænomena*.

The firſt ſight I had of this Appearance, was about half an hour after Seven of the Clock; at which time it had nothing remarkable to diſtinguiſh it from thoſe others which had been obſerved almoſt every Evening for ſome time, except a dusky Redneſs ariſing from the Weſtern Extremity of the luminous Arch; and that at the ſame time there was ſeen another like hazy Arch low to the Southward, fainter, but more ſteady than that to the North. I judg'd the higheſt Point of it to be ſomething more elevated than the Sun at Noon about the Winter Solſtice.

In a ſhort time after, the Northern Arch was riſen conſiderably higher from the Horizon, and continued to advance towards the Zenith, till 8; when in one part it paſs'd among the uppermoſt Stars of *Caſſiopeia*, and in another cloſe below the bright Star in the *Harp*.

The

The Heavens underneath look'd clear, and of a dark Blue, having no reſemblance either of Dawn or dusky Cloud, and the Pyramids of Light ſeem'd to ſpring immediately out of the pure Sky. The Arch it ſelf was very irregular, being full of Notches, ſome greater, ſome leſs. The dusky Red on the Weſt was changed to a light Crimſon, and was anſwer'd by the like Colour on the Eaſt. The Rays iſſuing from both Extremities, were thick and bright, appearing as if there were ſeveral one behind another. They were alſo generally longer than the reſt, and pointed conſiderably to the South of the Zenith. After 8, the Northern Arch retir'd again downwards, till it came among the Stars of the *Great Bear*; when the whole Scene was changed on a ſudden, and Rays were darted up from all ſides, and form'd that Crown-like, or Star-like Figure which has been ſufficiently deſcrib'd.

The intermediate *Area* (left between the innermoſt Extremities of the Rays coming from different Quarters, which very rarely, if ever, joyn'd) was of an irregular Figure, commonly inclining to an Oval, whoſe longeſt Diameter lay Eaſt and Weſt. Sometimes it appear'd as clear Sky, at other times was fill'd with a thin white Cloud, and that Cloud was often divided into two Parts, by an uneven crooked Line, running likewiſe Eaſt and Weſt.

The Rays which immediately ſurrounded this void Space, were of no great Length, and very unſtable: Yet two or three times, when they continu'd ſteady enough to afford an Opportunity of conſidering them attentively, their outermoſt Extremities were ſenſibly carried Southwards, the Center it ſelf remaining, to appearance, fix'd.

The

The Southern Quarter was fill'd with continual Flaſhings of Light. Theſe followed one another very quick, and were propagated upwards from the aforemention'd Arch with great Swiftneſs, each of them leaving in ſome parts of the Space it paſs'd through, a faint, and very tranſient Whiteneſs, which preſently vaniſh'd, and was quickly renew'd, uſually in the very ſame Track, by the next ſucceeding Flaſh. Yet none of thoſe Tracks were in any degree direct and uniform; but all very irregular and broken.

The Central Figure ſometimes diſappear'd for a while, and then returned again. Whether it always retained the ſame Situation with reſpect to our Horizon, I cannot depend on the Exactneſs of my Obſervations enough to determine. They were as follow.

About half an hour paſt Eight, the Center, as well as I could judge by my Eye, was very near a Star of the fifth Magnitude, placed by *Hevelius* at the End of the *Lizard*'s Tail, whoſe preſent Right Aſcenſion is about 331°, and Decl. 36° and an half N. At Nine, it was at the Northern Point of an *Iſoſceles Triangle*, whoſe Baſe was a Line joyning the Star in *Pegaſus* Shoulder, call'd *Scheat*, and the brighteſt of thoſe in his Knee; the Perpendicular from the Center being in proportion to the Baſe, about as 3 to 2. At 9h. 15′ the *Triangle* made between that and the two forementi-on'd Stars was become Right-angled at *Scheat*, the Diſtance being not much alter'd. At Ten, it was directly between the Zenith and *Andromeda*'s Head, at a Diſtance from this Star not ſenſibly different from what it had kept from the Northermoſt of the Two forementioned.

Ac-

According to the firſt of theſe Obſervations, the Central Point muſt have been very near the Meridian, and about 15° South of the Zenith of the Place where I was; which is a few Minutes directly North from *London*. The three laſt agree pretty well with one another, to carry it between 2 and 3 Degrees further Southward, and to give it a perpendicular Diſtance of 3 or 4 Degrees of a great Circle from the Meridian Eaſtward.

In the remarkable Appearance of this kind which happen'd the 6th of *March* 171$\frac{5}{6}$, I obſerv'd the like Center at near half an hour after 7 o' clock, to be ſomething nearer the Zenith, than the bright Star in the Northern Head of the *Twins*, and to be more Eaſterly by about half the Diſtance between that and the Star in *Pollux*'s Head. By comparing this Obſervation with the Situation of the Star at that time, the Center appears to have been about 16 or 17 Degrees from the Zenith, and about 2 or 3 diſtant from the Meridian Circle towards the Eaſt.

VII. *De eodem Phænomeno* Genevæ *viſo, Epiſtola Viri Clariſſimi Domini* Johannis Ludovici Calandrini, *Math. Prof. Ordinar.* Genevæ, *ad* Jac. Jurin, *R. S. Secr.*

Datam Genevæ *Kal.* Nov. 1726.

DIE Saturni $\frac{19}{8}$ Menſis Octobris, horâ ſextâ & dimidiâ veſpertinâ, Aurora Borealis fulgere cœpit, Phænomenon vobis quidem ſatis familiare, nobis verò ex ſeniorum ſententiâ plane novum, ſed tam illuſtre, & circumſtantiis à vulgaribus in Tranſactionibus ſæpe deſcriptis tam diverſum, ut de eo audiiſſe fortaſſe non pigebit.

Primò quidem, quâ horâ dixi, Arcus lucidus viſus eſt, cujus medium ſtabat in Horizontis Septentrione, extremitates ad Arcturum in Occaſu, & ad Pleiades in Ortu, 40° altus; lucidus ſatis, ut Solem è Septentrione jamjam ortum ire diceres; ſed hic nihil eſt novi.

Poſt horam ſeptimam, quaſi violenti incendii flammæ, vel continua fulgura totum albicantis arcus locum occupaverunt. Serpebant ex Horizonte ad cœli fornicem haud abſimiles veri furni flammis, extremitates arcûs rubicundo atrove fulgore vulgum terrebant, ipſæ flammæ in atrum fumum deſinere videbantur, ita ut diem Domini inſtare mulierculæ non dubitarent. Cœleſte hoc incendium pluſquam horam duravit; Scenâque mutatâ, ex Horizonte exilire columnas, quæ ad Zenith pertingerent, viſum eſt: Ex eâdem ſupradictâ baſi, coronâ in extremitate Phænomeni tum occidente & cornu Tauri inferiore ab alterâ parte ſurgente, columnæ plus minuſve latæ, 60 aut 70° altæ, tria vel qua-

quatuor minuta durantes, paulatim evaneſcentes, vel prius in igneum colorem verſæ ; poſt aliquod tempus ceſſarunt columnæ: tum Aurora Borealis ut primum fulſit, cui ante decimam ſucceſſerunt flammæ vehementiores quam prius, quibus ceſſantibus columnæ circa undecimam viſæ ſunt, magis diutinæ, & 120ſ plerumque altæ : Rurſus Aurora, mox flammæ, poſtea columnæ, atque ita ad tertiam uſque, quâ videbantur adhuc Phænomeni relliquiæ.

Ultra luculam, quæ ſemper Septentrionalem, quam dixi, cœli partem illuſtrabat, majoris Urſæ ſtellæ parum fulgebant. E Veſontio, quod 90 milliaribus hinc diſtat, eodem modo viſum fuiſſe Phænomenon accepimus.

Arſit interea unum ex illis Meteoris, ſtellæ cadentes dictum, quod ultra Phænomenon arſiſſe viſum eſt.

Ex reflexione Solaris luminis à partibus Atmoſphæræ Borealibus congelatis Auroras Boreales formari dictum eſt : Sed flammas adeò notabiles quo pacto explicentur non video. Si ab exhalationum incendiis ortum hoc Phænomenon cenſeatur ; Aurora Borealis Phænomeni comes, columnæ, duratio Phænomeni, & ipſius in eodem loco ſtatio, negotium faceſſent.

Joh. Lud. Calandrini,

VIII. *An Account of a præternatural Bony Subſtance found in the Cavity of the* Thorax, *communicated in a Letter to Dr.* Rutty, *by Mr.* William Giffard, *Surgeon.*

ON *Tueſday*, *May* the 10*th*, 1726, I open'd the Body of a Perſon, who dy'd the foregoing Day of a *Peripneumonia*; in the right ſide of whoſe *Thorax* I found an oſſeous Subſtance about the fourth of an Inch thick, ſix Inches long, and three broad, extending it ſelf under the third, fourth, fifth and ſixth Ribs, cloſely and ſtrongly connected to the *Perioſteum* of the Ribs, and the *Tunica propria* of the inner Intercoſtal Muſcles, by Fibres which ſhot from a ſtrong and thick intervening Membrane, which cloſely adher'd to its outer Surface or Back, and was continu'd over its inner likewiſe, thereby forming a *Capſula* for this præternatural Subſtance. Its upper Edge lay immediately below, and was contiguous to that Part of the Ribs, where they become cartilaginous: The Membrane that adher'd to, and cover'd it, continu'd thick ſome diſtance from it, and gradually grew thinner, and was at length loſt in the *Pleura*; from whence I judg'd this extraneous Body to have been form'd between the two *Lamellæ* of that Membrane. The Lungs ſo ſtrongly adher'd to its Inſide, that upon ſeparating them, part remain'd to it: Upon cutting into which, I obſerv'd all the Cells ſtuff'd with a thin, but ſomewhat digeſted *Pus*.

Upon farther Enquiry, I found on the ſame ſide, towards the Back, another Subſtance, but perfectly Bone, inveſted as the former, with a ſtrong and thick Membrane

brane, and by the Fibres which ſhot from it, tied to the Body of the *Vertebræ*, and the fourth, fifth, ſixth and ſeventh Ribs, and Intercoſtal Muſcles.

Its Back or Outſide was convex, agreeable to the concave Surface of the Ribs, which had made Indentations on that part of it where they preſs'd: Its Inſide was concave as the Ribs; the right Lobe of the Lungs ſtrongly adhered, part of which for that reaſon remained to it after Separation: Its Length was about ſeven Inches, Breadth about three from Edge to Edge: Its Thickneſs, in ſome parts a third, in others a fourth of an Inch. It had, as the former, a thick Membrane running from it, which gradually thinning was at length loſt in the *Pleura*; from whence I judge this extraneous Subſtance to have been formed as the forementioned.

" Since Mr. *Giffard* has not inſerted the Particulars " of this Gentleman's Indiſpoſition, I ſhall add, that " upon Inquiry, I have ſince been inform'd, that he " was troubl'd for ſome Years with a ſhort Cough, " which latterly was accompanied with a difficulty " of Breathing, and great Labour in Inſpiration; and " ſome time after, with a Weight and Pain on the " right Side; which Symptoms encreaſing more and " more, brought on the *Peripneumonia*, of which he " died in a few Days. The Riſe and Succeſſion of " thoſe different Symptoms proceed ſo naturally " from the Formation and Growth of this præterna- " tural Body, as well as the particular part of the " *Thorax* where it was lodg'd, that it is entirely need- " leſs to ſay any thing more upon them.

W. Rutty.

IX. *An*

IX. *Obſervatio de Succinea Gleba, Plantæ cujuſdam folio imprægnata, rariſſima. Auctore* Dno. Johanne Philippo Breynio, *M. D. R.S.S.*

INter Corpora Naturalia Succinis à Natura incluſa, illa longe rariſſima eſſe, quæ Vegetabili Regno ſuam debent originem, confitebitur mecum, opinor, quicunque Naturæ Curioſorum Pinacothecas, inque iis Succineas Gazas, non levem ipſis conciliantes ſplendorem, accuratius inſpexerit, Authoreſque de his rebus tractantes diligentius excuſſerit. Hæc inter autem cæteris Plantarum perfectiorum partes, ut folia, ſeminum receptacula, flores, ſi qui inveniuntur, &c. raritatis palmam præripiunt.

Hujus rei ratio procul dubio eſt, quod ex recentiorum mente, Succinorum officina naturalis, loca ſint ſubterranea, quorſum partes Vegetabilium, utpote terræ ſuperficiem inhabitantium, ægre et non niſi caſu, eoque rariſſimo pertingere poſſunt; cum Inſecta, quamvis etiam in aere viventia, tamen, ut ſe à frigore, aliiſque aeris injuriis defendant, vel alia etiam de cauſa, ſponte ſæpius rimas, hiatus et cryptas quærant et ſubeant ſubterraneas, & ad ſepulchrum properent, ubi à Succino adhuc liquido irretiuntur, involvuntur, ſuffocantur, cumque eodem in ævum duratura rigeſcunt.

Ejuſmodi glebam, quæ folium hujus notæ in ſinu ſuo fovebat, nuper Menſe Octobri præterito mihi exhibuit & examini ſubjecit meo hic *Gedani* Mercator quidam natione *Britannus*, *Philippus Benlows*, qui eandem,

dem, inter alia Succina nactus, maximi, imo pluris quam triginta aureorum æstimabat.

Hæc ovalis ferme, sed compressæ erat figuræ, & magnitudinis quæ in Icone Fig 11. adjuncta exprimitur, quartam Unciæ partem crassitie æquans, illius generis, quod falernum, à similitudine vini, salutatur Succinum, & quidem satis perspicua & pura, neque vel leviſſima fraudis macula conspurcata. Includebat per totam ejus mediam longitudinem exporrectum Folium aliquod Botanicis pennatum, quibusdam etiam alatum, quamvis minus recte, dictum; quod eleganti sane spectaculo, obscurum quidem, sed aureo colore coruscans, à reflectione & refractione radiorum luminis, oculis spectantium clare sese offerebat. Folium hoc non integrum, sed utraque extremitate mutilatum erat, ut ipsa indicat figura, quinque constans foliolorum oblongorum, utrinque non nihil acuminatorum paribus s. conjugationibus, quorum quædam erant ex parte exesa et mutila, in costa communi æque fere à se invicem distantibus. Situm erat in plano horizontali, quod in eo foliorum genere familiare est, præterquam, quod foliola obliquitate aliquantulum ab eo deflecterent; nequaquam vero conjugationes foliolorum decussatim positæ vel ullo modo videbantur, quod in foliis, quæ conjugata Botanicis dicuntur, semper obtinet; ut adeo folium hoc ex compositorum pennatorum genere esse, nullum mihi amplius restet dubium. Cujus autem exacte Plantæ speciei sit determinare vix licet; quia multæ species hujus familiæ, foliis vestiuntur sibi adeo similibus, ut etiamsi recentia, difficile sit à se invicem distinguere; accedit, quod nec venulæ foliolorum, vel oculis Microscopio armatis appareant, utpote quæ à Succino olim liquido obliteratæ & quasi incrustatæ erant. Quam proxime autem accedit ad Securidacæ secundæ, *Clusii*, s. Coronillæ

nillæ herbaceæ, &c. *Tournefortii*, quæ in dumetis *Pruſſiæ* ſatis familiaris eſt. Ab altera parte, inter bina foliola, aranea ſatis clare conſpicitur; ab altera vero parvula muſca, ſed hæc non niſi oculo lente armato.

Non memini me apud ullum Authorem folii hujuſmodi in Succino pennati inveniſſe mentionem, præterquam apud *Michaelem Mercatum* in nobili iſto opere Metallothecæ Vaticanæ titulo inſcripto, & immortalibus Archiatri Pontificii *Johannis Mariæ Lanciſii* meritis à ſitu, oblivione & interitu liberato. Ubi, pag. 89. inter alias elegantium glebarum Ranam, Piſciculum, Lacertulam variaque Inſecta continentium Icones, una etiam reperitur, quæ folium ejuſmodi pennatum paulo minus, ſed ob integritatem rarius & elegantius, octo conjugationum, foliolo extremo impari coſtam claudente, includit; quod parvum & tenellum Coronillæ herbaceæ, flore vario, *Tournefortii*, folium egregie exprimit, quamvis etiam non male ad Onobrychidem ſecundam, *Cluſii*, quæ pariter *Pruſſiæ* indigena eſt, referri poſſet.

Idem *Mercatus* aliam ibidem delineat glebam parvo in tenuia ſegmenta diſſecto folio, plantæ cujuſdam, forte umbelliferæ, impræ gnatam.

Interim tamen cum glebas memoratas, in laudato *Mercati* libro ſaltem, ſummo licet ſtudio delineatas, non vero ipſas viderim, præterea figuræ Ranæ majoris, Lacertulæ & Piſciculi, non levem mihi artificioſæ fraudis excitent ſuſpicionem, ego equidem pro genuitate earum vadium in me ſuſcipere nollem; quia notum eſt affatim, Artifices, ejuſmodi res adeo artificioſe Succinis poſſe includere, iiſque non nimium Curioſis imponere, ut non niſi ab expertiſſimis & oculatiſſimis poſſit detegi fusus.

X. *Obſervatio Defectus Solis habita* Patavii *VII. Cal.* Octob. 1726. *a Viro Cl. Domino* Johanne Poleno, *Aſtr. Prof. R. S. S.*

Temp. Appar.
H. ′ ″

5 25 25 Initium Defectus hoc tempore nullum apparebat: tum poſt tempus hoc ingruerunt denſæ nubes.

Dig. Obſcur.

29 5 1

31 48 Tegitur maxima earum, quæ conſpiciebantur in Sole, Macula, intermedia inter minores duas alias

Dig. Obſcur.

34 27 2
40 43 3
44 20 $3\frac{1}{2}$
47 15 4
48 12 $4\frac{3}{4}$

49 Creſcebat certe adhuc Obumbratio in Solari Diſco, ut trans nubes apparebat: mox vero cum denſiores fierent nubes, atque Sol ad Finitorem properaret, nihilquicquam amplius licuit obſervare,

XI. *Observatio Defectus Lunæ habita (tubo optico optimæ notæ, longo pedes* Parisienses *septem)* Patavii *VI. Id.* Octob. 1726. *ab eodem Viro Clarissimo.*

Temp. Appar.
H. ′ ″

16 16 44 Penumbra diluta.
18 54 Penumbra densior.
21 19 Umbra ad Lunæ limbum.
31 35 Attingit Mare Humorum.
35 47 Attingit Grimaldum.
38 34 Distat a Tychone diametro Tychonis ipsius, & Grimaldum tegit tertia ejusdem Grimaldi parte.
50 40 Fere attingit Pitatum.
17 3 41 Lansbergium tegit.
7 45 Attingit Reinoldum.
15 56 Attingit Fracastorium, & Galileum.
25 53 Attingit Mare Fœcunditatis.
39 6 Umbra proxima est ad Reinoldum, tegitque partem tertiam Maris Fœcunditatis.
46 Grimaldus emergit.
54 53 Grimaldus jam distabat ab umbra diametro majore sui integra.
18 5 44 Gassendus totus modo extra umbram, Mare Fœcunditatis dimidia circiter parte detectum, inter dehiscentes nubes videbantur.

Quæ nubes ad Finitorem deinde coactæ occiduam Cæli partem penitus obumbravere; neque Luna posterius apparuit.

XII. *Two newly diſcover'd Arteries in Women, going to the* Ovaria. *By Mr* Ranby, *Surgeon to his Majeſty's Houſhold, F. R. S.*

SOmetime ago I preſented the *Royal Society* with a Deſcription of two * Arteries, which ariſing from the *Aorta*, and ſending Branches to the *Glandula Renalis*, then deſcend to the *Teſticles*. And I have ſince found the ſame Arteries in Women, deſcending in the ſame common *Capſula* with the Spermatick Artery and -Vein, to the *Ovaria*. Theſe Arteries very probably are what the late Excellent Anatomiſt *Valſalva*, took for excretory Ducts of the *Glandulæ Renales*, the Diſpoſition and Progreſs of theſe being very much alike to what has been aſcrib'd to thoſe ſuppoſed Ducts.

* *v. Phil. Tranſ.* N°. 387.

FINIS.

Fig. 1.

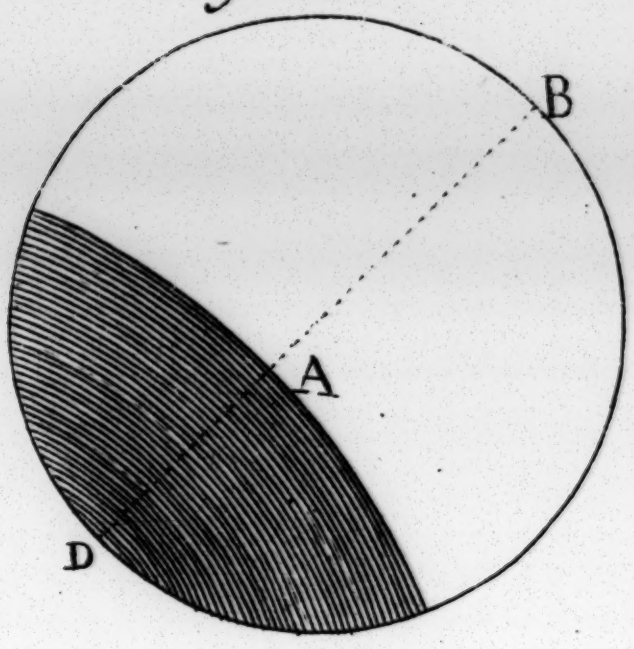


Fig. 2.

Fig. 3.

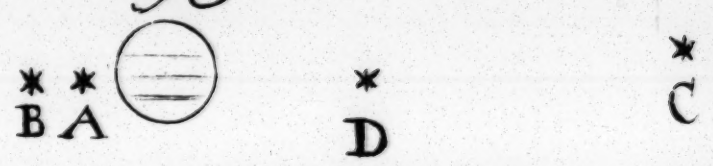


Fig. 4.

Fig. 5.

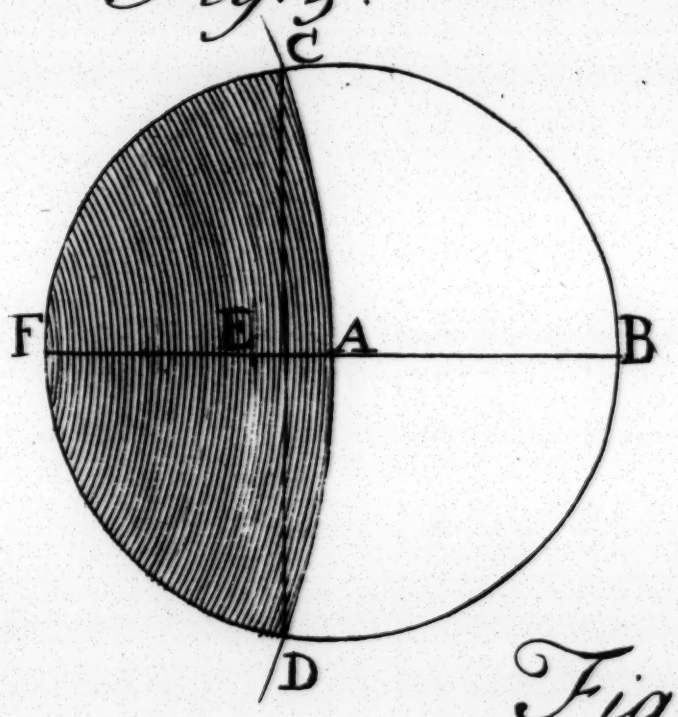


Fig. 6.

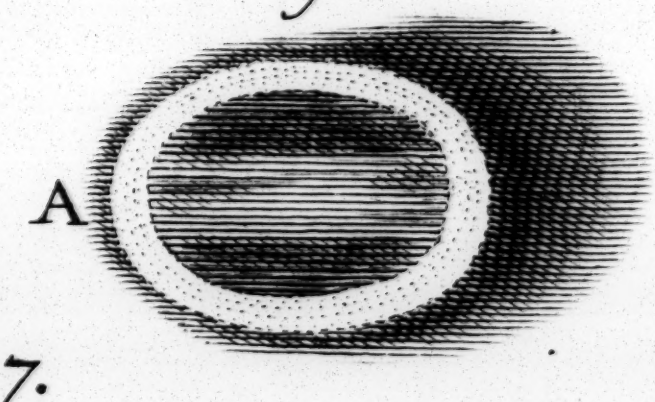
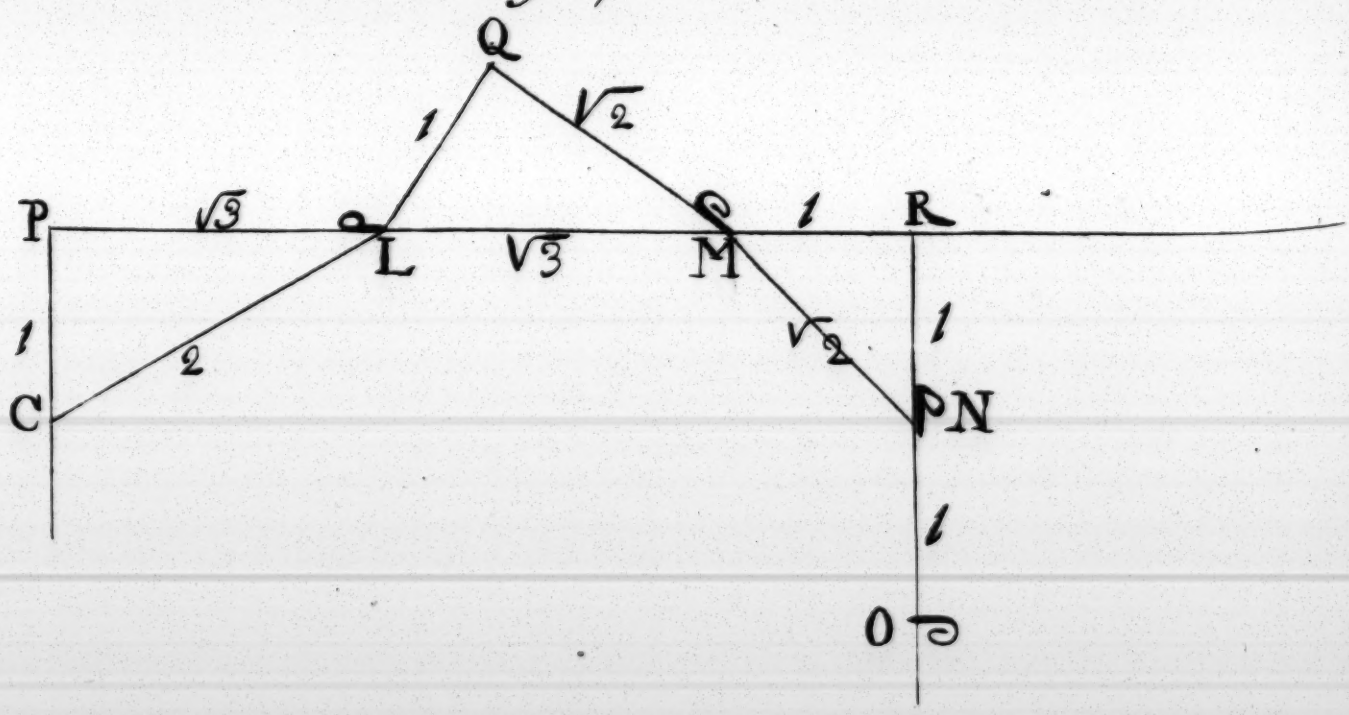


Fig. 7.



Numb. 396.

PHILOSOPHICAL TRANSACTIONS

For the Months of *November*, and *December*. 1726.

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Jos: Banks

I. *A ſhort Account of Negativo-affirmative Arithmetick, by* Mr. John Colſon, F. R. S.

THE Uſefulneſs of this Arithmetick conſiſts in this, that it performs all the Operations with more Eaſe and Expedition than the common Affirmative Arithmetick, eſpecially in large Numbers: And it differs from the common Arithmetick chiefly in this, that it admits of Negative Figures promiſcuouſly with the affirmative. Theſe negative Figures are diſtinguiſh'd from the Affirmative, by the Sign - placed over them. Thus $3\bar{7}09\bar{2}8\bar{6}57\bar{3}96\bar{1}47\bar{2}$ is one of theſe Numbers, which may be converted into its Equivalent common Number 2308726432039468, in this manner:

$$\begin{array}{r} 3009006503000470 \\ 0700280070961002 \\ \hline 2308726432039468 \end{array}$$

(1.) Write down all the affirmative Figures by themſelves, putting a Cypher in the place of every negative Figure. (2.) Write down all the negative Figures by themſelves, putting a Cypher in the place of every affirmative Figure. (3.) Subtract the laſt Number from the firſt, and the Remainder will be a common Number, equivalent to the given negativo-affirmative Number. See the Operation above.

But the readieſt practical Way of performing this Reduction in any given Number, will be in this manner: Begin at the left hand; and going over all the Figures in order, obſerve theſe Rules. (1.) An af-

 firmative

firmative Figure before a Negative muſt be diminiſh'd by an Unit. (2.) A negative Figure before an Affirmative, muſt be changed into its Complement to 10. (3.) A negative Figure before a Negative, muſt be changed into its Complement to 9. All other Figures muſt remain unchanged, and a Cypher is always to be underſtood where there is no ſignificant Figure. The ſign of the Cypher is neglected; but where there is occaſion to conſider it, it is always ſuppos'd the ſame as the Sign of the following Figure. Thus the negativo-affirmative Number $7\bar{2}9\bar{5}8\bar{6}4\bar{5}5\bar{9}8\bar{2}00\bar{1}730$ is immediately reduc'd to $710\bar{5}85\bar{5}4\bar{5}97\bar{7}99\bar{8}330$; and ſo of all others.

But on the contrary, common Numbers may be reduced to negativo-affirmative Numbers a great variety of Ways, by ſubſtituting inſtead of the Figures 1, 2, 3, 4, 5, 6, 7, 8, 9, their reſpective Values $\bar{1}9$, $\bar{1}8$, $\bar{1}7$, $\bar{1}6$, $\bar{1}5$, $\bar{1}4$, $\bar{1}3$, $\bar{1}2$, $\bar{1}1$, in any places at pleaſure. But the moſt uſeful Reduction of this kind is what I call *A Reduction to ſmall Figures*, which conſiſts in throwing out all the large Figures, 9, 8, 7, 6, out of any given Number, and introducing in their room the equivalent ſmall Figures $\bar{1}1$, $\bar{1}2$, $\bar{1}3$, $\bar{1}4$, reſpectively. Thus 182937462 may be reduc'd to $22\bar{3}1\bar{4}3\bar{5}42$, conſiſting only of ſmall Figures. But this Reduction may be perform'd more readily by theſe Rules following.

(1.) A ſmall Figure before a large Figure muſt be increaſed by an Unit. (2.) A large Figure before a large Figure muſt be changed into its negative Complement to 9. (3.) A large Figure before a ſmall Figure muſt be changed into its negative Complement to 10. Other Figures are not to be changed; and 5 will be ambiguous, being to be eſteemed either large

or

or ſmall, according as the Figure following is either large or ſmall. Some Examples of this Reduction ſhall here follow, both in whole Numbers and Decimal Fractions.

$$37068259764 = 4\bar{3}1\bar{3}2\bar{3}40\bar{2}44$$
$$729528960739957 = 1\bar{3}3\bar{1}5\bar{3}\bar{1}0\bar{4}1\bar{3}400\bar{4}\bar{3}$$
$$9260872395{,}87294 = 1\bar{1}3\bar{4}1\bar{1}\bar{3}240\bar{4}{,}\bar{1}\bar{3}3\bar{1}4$$
$$\text{Or (9) } 926087239587294 = \text{(10) } 1\bar{1}\,3\bar{4}1\bar{1}\bar{3}240\bar{4}1\bar{3}\,3\bar{1}4$$
$$\text{(m) } 3879164079\bar{5}3, \&c. = \text{(m) } 4\bar{1}21\bar{2}44\bar{1}21\bar{5}3, \&c.$$

It is to be obſerved, that in this laſt Example the Numbers are what I call *interminate*, or Approximations only; that is, the firſt and moſt valuable Figures are expreſs'd, and all the reſt (whether finite or infinite in Number, whether known or unknown) are omitted as inconſiderable, and inſinuated by the Mark &c. Alſo the Index *m* before the Number ſtands for ſome Integer, expreſſing the Diſtance of the firſt Figure 3 or 4 from the Place of Units; which Integer is either affirmative or negative, according as the ſaid firſt Figure ſtands in integral or fractional Places. The Example immediately before is a particular Inſtance of this.

And thus much by way of Notation: To proceed therefore to the Operations to be performed with theſe Numbers, whether reduced to ſmall Figures or not; and firſt of Addition.

Place the Numbers to be added juſt under one another, obſerving the Homogeneity of Places, as in common Numbers. Then beginning at the Right Hand, collect the Figures in the firſt Row or Column, according to their Signs, and place the Reſult underneath:

neath: And ſo ſucceſſively of all the other Columns, as in Example 1.

But if at any time this reſult cannot be expreſs'd by a ſingle Figure, it may be writ down with two or more Figures, obſerving the Homogeneity of Places, and then the Sum may be collected over again. But to ſave this trouble, it will be ſufficient to reſerve the Figure in mind which belongs to the next Column, and collect it with the Figures of that Column; as in Examples 2, 3.

If the Numbers to be added are reduced to ſmall Figures, as in Example 3, their Addition will be very ſimple, and the Sum may alſo be exhibited in ſmall Figures, by an eaſie Subſtitution of Equivalents, where there is occaſion.

(1.)	(2.)
257384263	647039682
70982 1370	49827365 1
5807305	81940376 5
953364212	1864643894

(3.)

(*m*)	2153140431213, &c.
(*m*)	5042031425512, &c.
(*m*-1)	4310231024113, &c.
(*m*-2)	5134211032 1, &c.
(*m*-3)	2130421032, &c.
(*m*-4)	1320212224, &c.
(*m*-5)	1322431 5, &c.
(*m*+1)	1333321413431 2, &c.

Sub-

Subtraction in this Arithmetick is reduced to Addition, by changing all the Signs of the Number to be ſubtracted.

Thus, if from (*n*) $7\bar{2}9\bar{3}8\bar{4}2\bar{9}6\bar{3}7$, &c. we are to ſubtract (*n*-2)$8\bar{1}07\bar{3}5\bar{9}2\bar{6}$, &c. the Remainder will be found as in Example 4.

(4.)

(*n*)	$7\bar{2}9\bar{3}8\bar{4}2\bar{9}6\bar{3}7$ &c.
(*n*-2)	$\bar{8}1\bar{0}7\bar{3}5\bar{9}2\bar{6}$ &c.
(*n*)	$7\bar{3}747\bar{1}\,5\bar{4}3\bar{4}3$ &c.

Thus in all Caſes will Addition and Subtraction be eaſily performed: But the chief uſe of this Method will be, to eaſe the trouble of prolix Multiplications. And here, as well as in Diviſion, the firſt and moſt valuable Figures may be firſt found, and conſequently the Product may be continued to as many Places as ſhall be required, without finding any unneceſſary Figures; which is a convenience not to be had in the ordinary way of Multiplication.

Let it be propoſed to Multiply together the Numbers 860572939871 5 and 3891758364 38, which reduced to ſmall Figures will be $1\bar{1}4\bar{1}43\bar{3}1\bar{4}0\bar{1}3\bar{1}5$ and $4\bar{1}\bar{1}2$ $2\bar{4}2\bar{4}44\bar{4}2$. Write down theſe two Numbers one under the other upon a ſlip of Paper, with the Figures at equal diſtances, and then cut them aſunder. Take either of the Numbers for a Multiplier, and place it over the other in an inverted poſition, ſo as its firſt Figure

Figure may be juſt over the firſt Figure of the Multiplicand.

Moveable Multiplier
4 1 1 2 2 4 2 4 4 4 2
- - --- --

Multiplicand
1 1 4 1 4 3 3 1 4 0 1 3 1 5

4 5 6 1 7 9 1 8 2 5 4 9 8 6 4 5 0 6 2 6 0 6 0 8 0
1 1 1 1 3 1 1 2 4 2 6 3 2 1 2 3 1 1

Product = $4\bar{6}5\bar{0}8\bar{6}19370\bar{9}6\bar{0}1\bar{7}07\bar{2}6\bar{2}3170$

Then Multiply theſe two firſt Figures together, and their Product ($4 \times 1 = 4$) place underneath. Then move your Multiplier a place forwarder, ſo that two of its firſt Figures may be over two of the firſt Figures of the Multiplicand; and collecting their two Products ($4 \times \bar{1} + \bar{1} \times 1 = \bar{5}$) put their Reſult underneath in the next place. Move the Multiplier a place forwarder; and collecting the three Products ($4 \times \bar{4} + \bar{1} \times \bar{1} + \bar{1} \times 1 = \bar{1}\bar{6}$) put the Reſult underneath, as in the Example. Move the Multiplier, and collect the four Products ($4 \times 1 + \bar{1} \times \bar{4} + \bar{1} \times \bar{1} + 2 \times 1 = 11$) which write underneath as before. And ſo proceed by one ſtop at a time, as long as any Figures of the Multiplier can be over any Figures of the Multiplicand. Laſtly collect the Product into one Line, which being reduced to a common Number will be 3349141936903996927377170.

From this Proceſs it may be obſerved, that at every new Situation of the moveable Multiplier thoſe Figures only are to be Multiply'd together, each by each, as are found over one another. And this Multipli-

Moveable Multiplier
$\bar{4}\,1\,\bar{2}\,\bar{2}\,\bar{4}\,\bar{4}\,\bar{4}\,\bar{2}\,1\,\bar{1}\,\bar{4}$

Multiplicand
1 1 4 1 4 3 3 1 4 0 1 3 1 5

3 3 4 9 1 4 1 9 3 6 9 0 3 9 9 6 9 2 7 3 7 7 1 7 0 = *Product*

Place the moveable Multiplier inverted in such a manner, as that its last Figure $\bar{2}$ may be just over 5 the last Figure of the Multiplicand. Multiply these together ($\bar{2} \times 5 = \bar{10}$) and set down the last Figure of the Product 0 just under, reserving the first Figure $\bar{1}$ for the next place. Then move the Multiplier a place forwarder, so that two of its last Figures may be over two of the last Figures of the Multiplicand, and then Multiplying and collecting you will have $\bar{1} + \bar{2} \times 1 + 4 \times 5 = 17$. Set down 7 in the next place of the Product, and reserve 1. At the next remove you will have $1 + \bar{2} \times \bar{3} + 4 \times 1 + 4 \times 5 = 31$. Set down 1 and carry 3. Then $3 + \bar{2} \times \bar{1} + 4 \times \bar{3} + 4 \times 1 + \bar{4} \times 5 = \bar{2}\,\bar{3} = \bar{37}$. Set down 7 and carry $\bar{3}$. Then $\bar{3} + \bar{2} \times 0 + 4 \times 1 + 4 \times \bar{3} + \bar{4} \times 1 + 4 \times 5 = \bar{3} = \bar{17}$. Set down 7 and carry $\bar{1}$. And so proceed as long as there can be any Figures over one another, and the Product will be found as before.

This way of Multiplication is so easy, and may be made so familiar by a little Practice; that it will be but little short of *Multiplication by Inspection*, and will doubtless seem very surprizing to those who are only acquainted with the common tedious way of Multiplication: especially if we content our selves with a mental preparation of the Numbers given, or only mark those Figures that are to be changed, which by some Practice is easily attained.

The

tiplication is to be perform'd, and the ſeveral Products collected, according to the Rules of Specious Multiplication, wherein like Signs will make +, and unlike Signs will make — in the Product. This will always make the Products deſtroy one another, or at leaſt will depreſs and keep them low, and the Figures themſelves being always ſmall, the Reſult will be always ſmall, and often but a ſingle Figure, which is the great Compendium of this Method.

When an Approximation only is deſir'd, or when the Product is to be produced to a given Number of places, the Operation may be continued one place farther, in order to obtain ſo many Places true as are required. For ſeldom any Correction extends beyond the place immediately aforegoing, and that is generally corrected but by an unite, and very often needs no Correction at all; which will be of no ſmall convenience in the Multiplication of Decimal Fractions.

In this Method we may (if we pleaſe) begin the Proceſs of Multiplication from the loweſt places, or from the right hand, as is uſual in common Arithmetick, and then the Correction may be carry'd on continually to the next place, and ſo the Product may be always comprehended in one Line, without the uſe of any Superfluous Figures. Of this I ſhall give an inſtance in the foregoing Example.

The first of these two ways of Multiplication will be most convenient for interminate Numbers. As if we were to multiply (m) 307149741748, &c. by (n) 1836097126 49, &c. the Product will be found (m + n) 563956758222, &c. as may appear from the Proceſs following.

(n) 2244103134 51, &c.

(m) 3131503423 52, &c.

(m + n) 6440572583 78, &c.

Here the Index of the firſt Figure of the Product will be m + n, or the Sum of the Indexes of the given Numbers; but it would have been m + n + 1 if there had been any increaſe from the Product of the two firſt Figures, or if there had been any correction to have been made to the Cypher, which is underſtood before the firſt Figure of the Product.

When both the Numbers to be multiply'd are interminate, as in the laſt Example, they ought to conſiſt of the ſame number of Places, or otherwiſe the greater number muſt be reduced to the leſſer, by cutting off the ſuperfluous Places: And the Product is not to be continued beyond the ſame number of Places. If but one of the number is interminate, the other muſt be reduc'd to the form of an interminate Number, either by cutting of the exceſs of places if it has more, or by ſupplying or ſuppoſing Cyphers, if it has fewer places than the interminate Number. Then the ſame reſtrictions will take place as before.

The Method of Diviſion in this Arithmetick will not be ſo ſimple or expeditious as Multiplication. After a tryal of ſeveral ways, I think this following will be the moſt commodious. Reduce the Dividend and Diviſor

to ſmall Figures, and form a Tariffa or Table of all the Multiples of the Diviſor as far as 5. Compare theſe Multiples with the Dividend, and with the ſeveral Remainders after the Multiples have been Subtracted, by which means you will diſcover the ſeveral ſmall Figures and their Signs, to be put ſucceſſively in the Quotient.

To form the Table of Multiples, ſet down the Diviſor above, drawing a line, under which ſet down the Diviſor over again, putting 1 over againſt it. Add theſe two together according to the Rule for Addition in ſmall Figures, and put 2 over againſt their Sum. Add this laſt and the Diviſor together, and put 3 over againſt their Sum. Then add this laſt and the Diviſor together, and put 4 over againſt their Sum. Laſtly, add this and the Diviſor together, putting 5 over againſt their Sum. Thus will you have a Table of all the Multiples of the Diviſor, as far as will be neceſſary.

Thus for Example, if (m + n) 563956758222, &c. = (m + n + 1) 1444043242222, &c. is to be divided by (n) 1836097 12649, &c. = (n) 2244 1031345 1, &c. the Proceſs will be as here follows, by which the Quotient will be found (m) 3131503423 54, &c. = (m) 307149741746, &c.

TABLE of Multiples.

	(n) 2244 1031345 1, &c.
1	(n) 2244 1031345 1, &c.
2	(n) 4332 2142530 2, &c.
3	(n) 5512 3114215 3, &c.
4	(n + 1) 1334 4412514 04, &c.
5	(n + 1) 1122 0514432 45, &c.

Quo-

Quotient = (*m*) 3 1 3 1 5 0 3 4 2 3 5 4, &c.

Dividend = (*m* + n + 1) 1 4 4 4 0 4 3 2 4 2 2 2 2, &c.

5 5 1 2 3 1 1 4 2 1 5 3, &c.

1 3 1 3 2 4 2 0 3 3 5, &c.

2 2 4 4 1 0 3 1 3 4 5, &c.

1 5 2 3 3 4 5 1 0 1 0, &c.

5 5 1 2 3 1 1 4 2 1, &c.

3 3 5 1 4 0 4 1 1, &c.

2 2 4 4 1 0 3 1 3, &c.

1 1 1 3 3 3 1 3 2 4, &c.

1 1 2 2 0 5 1 4 4, &c.

1 5 3 4 2 2 0, &c.

5 5 1 2 3 1, &c.

1 2 4 4 5 1, &c.

1 3 3 4 4 4, &c.

3 2 0 5, &c.

3 3 3 2, &c.

1 5 3 3, &c.

5 5 1, &c.

1 2 4, &c.

1 1 2, &c.

1 2, &c.

The Index of the Quotient is found by ſubtracting the Index of the Diviſor from the Index of the Dividend, when the firſt Figure or Figures of the Diviſor are not greater than the like Figures of the Dividend. Thus $\frac{(m+n)\ 56,\ \&c.}{(n)\ 18,\ \&c.} = (m)\ 30,\&c.$ When they are greater, then an Unit muſt be farther Subtracted from the Index of the Dividend. Thus $\frac{(m+n+1)\bar{1}\bar{4}\bar{4},\ \&c.}{(n)\ \bar{2}2\bar{4},\ \&c.} = (m)\ 3\bar{1}\bar{3},$ &c.

Now a little to illuſtrate this Proceſs, it may be obſerved, that the Dividend $\bar{1}\bar{4}\bar{4}$, &c. or 56, &c. being compared with the ſeveral Multiples of the Diviſor in the Table, it is eaſily perceived that 55, &c. makes the neareſt approach to it, and therefore its correſpending Figure 3 muſt be made the firſt Figure of the Quotient, which I place juſt over. Under this I place its reſpective Multiple, changing the Signs and collecting, becauſe it ought to be ſubtracted. Then the remainder 131, &c. being compared with the Table, I find the neareſt to it (whether in exceſs or defect) to be $\bar{2}\bar{2}$, &c. or 18, &c. belonging to 1. This therefore is made the ſecond Figure of the Quotient, and its Multiple with the Signs changed is placed under, and collected with the laſt Remainder. The Reſult, or new Remainder $\bar{1}5\bar{2}$, &c. that is $-\ \bar{1}5\bar{2}$, &c. or $-\ 52$, &c. being compared with the Table, the neareſt Multiple is 55, &c. belonging to 3, which 3 therefore is made the next Figure of the Quotient, but with the Sign — over it, becauſe this Remainder is Negative. And for this reaſon the Multiple 55, &c. is put down in its place without changing its Signs. The reſt of the Proceſs will be obvious enough, and if any ſcruple ariſes about placing the Numbers, it may eaſily be removed

moved by a little attention to their reſpective Indexes.

In the Diviſion of interminate Numbers, the ſame reſtrictions are to obtain, as are already mention'd in Multiplication.

And this may ſuffice for a ſhort Summary of Negativo-affirmative Arithmetick, as to the ordinary Operations of Addition, Subtraction, Multiplication, and Diviſion. What improvements may be had from hence in the Extraction of Roots, whether of pure or affected Equations, I ſhall leave to future inquiry.

But all theſe Operations, whether in the common Decimal Arithmetick, or in this here compendiouſly deſcribed; and not only in Decimal Arithmetick, but in the ſeveral other Species, as Duodecimal, Sexageſimal, Centeſimal, *&c.* all theſe Operations (I ſay) may be much more eaſily and readily perform'd, and with equal accuracy, by an Inſtrument I have contrived call'd *Abacus*, or *the Counting-Table*, which I hope ſhortly to communicate to the inquiſitive in theſe matters, and by which all long Calculations may be very much facilitated. In the mean time this ſhort account of Negativo-affirmative Arithmetick, in Denary or Decimal Numbers, may be premis'd, by way of Introduction to the knowledge and uſe of the ſaid Inſtrument.

II. *Eclipſis Lunæ obſervata* Romæ, *ad radices Collis* Quirinalis, *nocte ſequente diem* 31. Octobris, 1724. *per clariſſimum Virum* Franciſcum Blanchinum. *Ex Epiſtola Rev. & Cl. Viri,* Johannis Baptiſtæ Carbone, *S. Jeſ. ad* Iſaacum Sequeyra Samuda, *M. D. R. S. S. Coll. Med. Lond. Lic.*

T. V. poſt Med. Noct.

	H.	*M.*	*S.*
INitium umbræ veræ (quantum ſecerni poteſt à penumbra) apparere nunc incipit in limbo Lunæ, in ea parte diſci, quam ſecat radius à centro Lunæ ductus per maculam Ariſtarchi.	3	15	40
Cum nubes redderent difficiliorem conſpectum macularum, curavi definire quantitatem diametri obſcuratæ ope Micrometri. Nunc igitur latent Digiti 3. circiter. Nam portio diametri A B *Fig.* 1. ab umbra libera, eſt partium 14, qualium tota Lunæ diameter eſt 18 $\frac{1}{2}$.	3	32	40
Serenato tantiſper cœlo, umbræ limes tranſire videtur per Rheinoldum. Portio autem Diametri A B ab umbra non infecta eſt partium Micrometri 13.	3	39	40
Umbræ limes tanto ſpatio eſt infra Grimaldum (in tubo invertente objecta) quanta eſt diameter ejuſdem maculæ Grimaldi. Tranſit quoque limes umbræ per Eudoxum & Ariſtotelem.	3	50	40

	H.	M.	S.
A B partes Micrometri 9.	3	54	40
Umbræ limes pervadit centrum Maris Criſium, & ſtringit limbum maculæ Grimaldi. Portio verò A B immunis ab umbra æquat partes Micrometri 8 $\frac{1}{2}$.	4	6	10
Sirius ad Meridianum appellit clariſſimè.	4	7	53
Totum Mare Criſium jam latere incipit, & in umbræ limite verſatur etiam centrum maculæ Grimaldi. A B eſt partium Micrometri 8.	4	12	40
A B partium Micrometri 7 $\frac{2}{3}$.	4	20	0
A B partium 7 $\frac{1}{4}$ circiter. Totus Grimaldus extra umbram, & Ricciolus quoque viſitur. Limes umbræ tranſit per marginem Maris Nectaris.	4	25	0
Detegitur Galilæus.	4	41	0
A B partes Micrometri 8.	4	52	0
Ariſtarchus incipit ex limite umbræ prodire.	4	55	30
Centrum Ariſtarchi exit.	4	56	30
Totus Ariſtarchus extra umbram.	4	57	30
Centrum Copernici exit.	4	59	20
Totus Copernicus extra umbram.	5	0	10
Eratoſthenes exit.	5	8	30
Helicon incipit emergere.	5	18	50
A B partes Micometri 12 $\frac{1}{3}$.	5	24	50
Plato incipit emergere.	5	25	50
Totus Plato extra umbram.	5	26	55
Plinius extra umbram.	5	28	20
A B partes Micrometri 14, vel 14 $\frac{1}{2}$.	5	33	50
Quare digiti tres adhuc latent.			
Prior limbus Maris Criſium incipit prodire.	5	38	50
Centrum Maris Criſium extra umbram.	5	40	50
Totum mare Criſium extra umbram.	5	44	50

Finis

II. *Eclipſis Lunæ obſervata* Romæ, *ad radices Collis* Quirinalis, *nocte ſequente diem* 31. Octobris, 1724. *per clariſſimum Virum* Franciſcum Blanchinum. *Ex Epiſtola Rev. & Cl. Viri,* Johannis Baptiſtæ Carbone, *S. Teſ. ad* Iſaacum Sequeyra Samuda, *M. D. R. S. S. Coll. Med. Lond. Lic.*

T. V. poſt Med. Noct.

	H.	*M.*	*S.*
INitium umbræ veræ (quantum ſecerni poteſt à penumbra) apparere nunc incipit in limbo Lunæ, in ea parte diſci, quam ſecat radius à centro Lunæ ductus per maculam Ariſtarchi.	3	15	40
Cum nubes redderent difficiliorem conſpectum macularum, curavi definire quantitatem diametri obſcuratæ ope Micrometri. Nunc igitur latent Digiti 3. circiter. Nam portio diametri A B *Fig.* 1. ab umbra libera, eſt partium 14, qualium tota Lunæ diameter eſt 18 $\frac{1}{2}$.	3	32	40
Serenato tantiſper cœlo, umbræ limes tranſire videtur per Rheinoldum. Portio autem Diametri A B ab umbra non infecta eſt partium Micrometri 13.	3	39	40
Umbræ limes tanto ſpatio eſt infra Grimaldum (in tubo invertente objecta) quanta eſt diameter ejuſdem maculæ Grimaldi. Tranſit quoque limes umbræ per Eudoxum & Ariſtotelem.	3	50	40

	H.	M.	S.
A B partes Micrometri 9.	3	54	40
Umbræ limes pervadit centrum Maris Criſium, & ſtringit limbum maculæ Grimaldi. Portio verò A B immunis ab umbra æquat partes Micrometri 8 $\frac{1}{2}$.	4	6	10
Sirius ad Meridianum appellit clariſſimè.	4	7	53
Totum Mare Criſium jam latere incipit, & in umbræ limite verſatur etiam centrum maculæ Grimaldi. A B eſt partium Micrometri 8.	4	12	40
A B partium Micrometri 7 $\frac{2}{3}$.	4	20	0
A B partium 7 $\frac{1}{4}$ circiter. Totus Grimaldus extra umbram, & Ricciolus quoque viſitur. Limes umbræ tranſit per marginem Maris Nectaris.	4	25	0
Detegitur Galilæus.	4	41	0
A B partes Micrometri 8.	4	52	0
Ariſtarchus incipit ex limite umbræ prodire.	4	55	30
Centrum Ariſtarchi exit.	4	56	30
Totus Ariſtarchus extra umbram.	4	57	30
Centrum Copernici exit.	4	59	20
Totus Copernicus extra umbram.	5	0	10
Eratoſthenes exit.	5	8	30
Helicon incipit emergere.	5	18	50
A B partes Micometri 12 $\frac{1}{3}$.	5	24	50
Plato incipit emergere.	5	25	50
Totus Plato extra umbram.	5	26	55
Plinius extra umbram.	5	28	20
A B partes Micrometri 14, vel 14 $\frac{1}{2}$.	5	33	50
Quare digiti tres adhuc latent.			
Prior limbus Maris Criſium incipit prodire.	5	38	50
Centrum Maris Criſium extra umbram.	5	40	50
Totum mare Criſium extra umbram.	5	44	50

Finis

		H. M. S.
	Finis umbræ veræ, quæ exit è limbo Lunæ circa punctum definitum per Diametrum à centro ductam per Cleomedem.	5 48 50

III. *Obſervationes aliquot Circum-Jovialium habitæ eodem anno* 1724. *per eundem Cl.* Blanchinum. *Ex eadem Epiſtola.*

Dies.	Menſ.		T.V. poſt Med.Noct. H. M. S.
8	Jun.	IN Caſtro Viſcardi ſupra Vulſinium in eodem ferè Meridiano cum Vulſiniis (vulgò Bolſena) Satelles ſecundus B (*Vide Fig.* 2.) ex umbra Jovis emerſerat, & ita configuratus viſebatur cum cæteris.	2 48 30
		Satelles tertius C ſubit limbum Jovis è directo faſciæ mediæ.	3 0 30
		Satellitis intimi A incipit lumen imminui.	3 26 20
		Totalis ejuſdem immerſio, reliquis Satellitibus B, D, & faſciis perſpicuè apparentibus.	3 27 10
24	Jun.	Romæ obſervata eſt tum immerſio ſecundi Satellitis, tum primi, ita configuratis Jovialibus. (*Vide Fig.* 3)	
		Satellitis ſecundi B incipit lumen debilitari.	1 39 0
		Omnimoda ejuſdem immerſio.	1 40 20

Etiam

Dies.	Menſ.		T.V. poſt. Med.Noct. H. M. S.
		Etiam Satellitis primi A lumen imminui jam incipit.	1 41 50
		Lux ejuſdem omninò diſparet, cæterorum lumine, & faſciarum adſpectu nitidiſſimè perſeverante.	1 42 50
Kal.	Jul.	Illuceſcente Aurora, Romæ immerſio totalis intimi Satellitis Jovis clariſſimè ſpectata eſt; cum ante minuta ſecunda horaria 55^{II} circiter, lumen ejuſdem cœperit debilitari, cæteris Satellitibus & faſciis perſpicuè apparentibus, cœlo clariſſimo.	3 24 29
			T.V. poſt Merid.
18.	*Aug.*	Romæ intimus Jovis Satelles ex umbra cœpit emergere, & pleniſſimè lucebat in A, inter limbum Jovis, & ſecundum Satellitem B, (*Vide Fig.* 4.) qui a limbo Jovis diſtabat ſemidiametro circiter Jovialis diſci. Satelles A emerſit ab umbra inter duas faſcias corporis Jovialis inferiores, ſitu everſo in Teleſcopio.	0 40 45
		Obſervatio fuit clariſſima & diligentiſſima, cœlo clariſſimo.	0 41 25
			T.V. poſt Merid.
25.	*Sept.*	Romæ, intimi Jovis Satellitis initium Emerſionis.	11 25 55
		Totalis recuperatio luminis.	11 27 5

Dies. Menſ.		*Temp. V. poſt Merid.* *H. M. S.*
11. *Octob.*	Albani, ejuſdem intimi Satellitis initium Emerſionis.	9 53 8

Ex pluribus obſervationibus Romæ habitis, & Ulyſſipone, eruitur Meridianorum differentia 1 h. 28′ 0″, ſeu Grad. 22, quæ ſanè differentia aut omninò vera, aut quam proximè veritati accedere videtur.

IV. *Obſervatio Lunaris Eclipſeos* Albani *habita poſt Occaſum Solis diei Dominicæ* 21 Octobris. *S. N.* 1725. *Ab eodem. Ex eadem Epiſtola.*

H. M. S.

HOrologiis pendulo inſtructis, & per dies plures ad meridiem verum exactis utebamur.

Occaſum ſolis ſpectare hodie non licuit, nubibus occupantibus occidentalem Horizontis plagam.

Nubibus pariter orientalem tractum obſcurantibus in Lunæ ingreſſu in umbram, tam pertinaciter auſtralis ventus novas cogebat, ut per totum tempus immerſionis integri globi Lunaris in umbram, vix ter, aut quater, idque raptim, tubum opticum in eum dirigere datum ſit.

Circa hoc tempus umbra videbatur ad centrum Lunæ pertingere. Sed cum maculæ diſtinctè definiri non potuerint breviſſimo illo ſpatio temporis, quo per nubium intervalla diſcus Lunaris detegebatur, præcisè noſcere non valemus hanc ipſam phaſin digitorum ſex Lunaris diametri obtectorum, licet paulò abludat à minuto 15, poſt horam ſextam à Meridie. 6 15 0

Totalis Immerſio ad hoc circiter minutum temporis referenda eſt, quantum ſpectare licuit ex duobus, aut tribus minutis horariis, quibus Lunam vidimus ſatis di- 6 45 0

H. M. S.

ſtinctè. Verùm ſecunda temporis definire, quibus umbra vera totum diſcum obtinuit, permiſſum nobis non fuit à nubibus ſuccedentibus.

Poſt immerſionem totalem diſcus Lunæ apparebat ab Atmoſphæræ terreſtris radiis refractis rubeſcens, dilutiori tamen colore in ea parte limbi, quam poſtremam Sol deſeruerat.

Inducta poſtmodum ſerenitate, licuit obſervationes emerſionis perficere.

Subalbicat diſcus Lunæ in limbo proximè illuminando. Nondum tamen lux directa Solis diſcum attingit. 8 20 0

Clarior adhuc fit limbus Lunæ; ſed nondum excedit ab umbra vera. 8 25 0

Nunc primùm limbus Lunæ incipit lumen recuperare in parte circumferentiæ ſita inter maculas Grimaldi & Galilæi: quæ maculæ adhuc latent. 8 27 0

Limes illuminationis attingit primum limbum Grimaldi. 8 29 40

Totus Grimaldus extra umbram. 8 30 40

Galilæus emergit ex umbra. 8 31 30

Ariſtarchus incipit emergere. 8 35 40

Totus Ariſtarchus emerſit. 8 36 0

Prior Copernici limbus incipit illuminari. 8 48 20

Totus Copernicus extra umbram. 8 49 50

Totus Plato emerſit. 8 51 20

Prior limbus Tychonis incipit emergere. 8 54 0

Totus Tycho extra umbram. 8 56 0

Subtenſa arcus C A D, & C F D per Micrometrum explorata eſt partium Micrometri 22, qualium Lunæ diameter eſt 24 in 8 59 0

tubo

	H.	M.	S.
tubo palmorum undecim Rom. A B verò eſt partium 12. (*Vide Fig.* 5.)			
Menelaus 25 exit. (25 eſt numerus maculæ aſſignatus in Lunæ imagine, à Pariſienſi Academia edita.)	9	2	0
Macula clarior ſita ante Plinium exit.	9	5	0
Hermes ab umbra prodit.	9	6	0
Plinius emergit.	9	50	0
Incipit emergere Poſſidonius 27.	9	16	0
Maris Criſium limbus prior emergit.	9	18	0
Totum Mare Criſium extra umbram.	9	25	0
Langrenus 39 exit.	9	24	0
Umbræ extremum in limbo Lunæ adhuc videtur.	9	24	30
Finis umbræ veræ.	9	25	0

V. *Conſpectus Maculæ Platonis in Luna cœlo clariſſimo nocte ſequente diem* 16 Auguſti 1725, *hora prima poſt occaſum Solis* Romæ *in monte* Palatino, *per tubum opticum* Joſephi Campani, *palmorum* 150 Romanorum, *ſpectante eodem Viro Cl. Ex eadem Epiſtola.*

MArgines elevati in ambitu maculæ perſundebantur luce Solis, & candorem conſuetum oſtendebant: fundus maculæ tenebroſus ſpectabatur, cum ad illum radii ſolares nondum pertingerent. Sed projectio lucis minus candidæ, imò nonnihil rubeſcentis, pervadebat mediam aream maculæ (ut in *Fig.* 6.) non ſecus, ac ſi in latere marginis A Soli obverſo foramen aliquod fuerit, per quod radius Solis admitteretur.

Cla-

Clariſſimus hic Vir in ſua, quam ad me ſcripſit, epiſtola, ratiocinari ſuper hoc maculæ conſpectu nonnihil voluit, ſuamque de ea re mentem exponere. Duplicem verò innuit cauſam, unde prædictus effectus procedere poſſet; vel ſcilicet foramen in latere marginis Soli obverſo; vel refractionem alicujus radii ſolaris in ſummitate marginis factam, unde interiores partes ipſius maculæ pervaderet radius. Utrumque ſanè probabile, & utrumque pariter confirmat, dari circa Lunam Atmoſphæram noſtræ non abſimilem, ut & ipſe Auctor infert; ſive enim foramen admittamus, per quod Solis radius introducatur, & hîc ſane neutiquam videri poſſet in obſcuro cavo maculæ, niſi ab exhalationibus lucem reflectentibus fieret conſpicuus; vel admittamus refractionem, & hæc ſine intermedio craſſiori dari nequit. Hæc addere volui, ut ratio innoteſceret, quæ laudatiſſimum Virum adduxit ad hujus maculæ obſervationem accuratè deſcribendam.

VI. *A Remark upon the New Opinion relating to the Forces of moving Bodies, in the caſe of the Colliſion of* Non-Elaſtic Bodies. *By Mr.* John Eames, *F. R. S.*

1. THE antient Opinion, and moſt generally entertained, is, that the Forces of Bodies in Motion are as their Quantities of Motion, *i. e.* as their Quantities of Matter multiplied by their reſpective Celerities.

2. The new Opinion is, that the Forces of Bodies in Motion are as their Quantities of Matter multiplied by the Squares of the Velocities; ſo that in equal Maſſes the moving Forces will not be as the Velocities themſelves, but as their Squares.

3. Hence it follows, that a Body moving with five Degrees of Velocity, moves with five and twenty Times more Force, than when it moves but with one Degree; whereas according to the common Opinion, it has but five times as much, *i. e.* proportional to its Velocity.

4. The lateſt Experiments brought to prove the truth of the new Opinion are made upon ſoft, or yielding Subſtances. Now theſe have been already obſerved to be a little complicated, and improper for this Purpoſe. The proper Uſe of Experiments of this kind being rather to diſcover, and ſettle the Laws, which ſuch kind of Subſtances obſerve in the Reſiſtance they make to Bodies moving in them, than the Forces themſelves, with which the Bodies move, which ought to be determined before hand by ſome ſimple Experiment, fit to determine that Matter.

5. A variety of Experiments have been made, and reaſoning uſed in *England* and *France*, to prove the Truth of the common Opinion; but they do not entirely ſatisfie all the Gentlemen on the other ſide of the Queſtion. The preſent ingenious Profeſſor of Mathematicks and Philoſophy at *Utrecht* tells us in the Preface to his *Epitome Elementorum Phyſico-Mathematicorum*, publiſh'd this Year, *Anno* 1726, *In corporum motorum viribus ſupputandis amplexus ſum ſententiam Cl.* Leibnitſii, Hugenii, Poleni, S. Graveſandii, & *antiquæ valedixi, quam hactenus foveram, & docueram: Neque me retinuerunt argumenta doctiſſimorum virorum in* Galliis & Britannia *eandem defendentium.*

Et quando experimenta à Poleno & 'S Graveſandio *deſcripta examinantur & inſpiciuntur, tam manifeſto evincunt vires corporum percutientium eſſe in ratione compoſita ex quadrata velocitatum, & ſimplici maſſarum, ut illis ſubſcribere teneamur, niſi apertiſſimis contradicere ſtudeamus.*

6. I beg leave to examine the truth of the new Opinion in the Caſe here propoſed, *viz. Vires corporum percutientium*; and I ſhall endeavour to ſhew from their own Principles, that it cannot be true in all the Caſes of Non Elaſtic Bodies.

7. 'Tis allow'd, that the common Rules of finding the Velocities of Non-Elaſtic Bodies after the Stroke are true: For thus the ingenious Mr. *S. Graveſand* tells us in Paragraph 251, of his *Supplementum Phyſicum*; *Ex hoc principio* (i.e. *multiplicando maſſam per velocitatem*) *deduxêre Philoſophi ipſas illas regulas* n. 234. *ſ.* 237. *ſ. quas nos variis modis ex principiis noſtris deduximus; mirum hîc quid contigit, error erroris fuit deſtructio, & duplex error ad veritatem conduxit; falſum de menſura virium ſecuti ſunt principium, & quod veritati etiam minimè congruum eſt, nullam vim intro premendo*

partes

partes, & harum ſuperando cohæſionem corpora amittere poſſuere.

8. Now the Rule for finding the common Velocity of Non-Elaſtic Bodies moving the ſame way after the Colliſion, is, to divide the Sum of the Quantities of Motion in the two Bodies, by the Sum of the Quantity of Matter.

9. 'Tis alſo granted, *Motu duobus corporibus communi corpora hæc in ſe mutuo agere non poſſe.* In Sect. 215. *Supplement. Phyſ.*

10. *Pendet ergo ictus à velocitate reſpectiva, qua manente intenſitas impactionis eadem erit, quomodocunque celeritates abſolutæ varient.*

11. *Ab intenſitate hac pendet partium introceſſio, quæ ergo ſemper eadem erit, ſi duo corpora eadem velocitate reſpectiva in ſe mutuo incurrunt, quibuſcunque velocitatibus moveantur.*

12. Theſe Principles furniſh us with an Argument againſt the new Opinion. For if it be true, then equal Cauſes may have unequal Effects, and that in their own ſenſe of an Effect: The Proof ſhall be taken from Inſtances of the Effects of the Colliſion of Non-Elaſtic Bodies, whoſe reſpective Velocities ſhall be always equal.

13. Let (A) and (B) ſtand for two Non-Elaſtic Bodies of equal Quantities of Matter; and let (B) be at reſt, while (A) moves towards it with 8 degrees of Velocity.

1. Here the common Velocity after the Stroke will be half the Velocity of (A) before the Stroke, *i.e.* 4 degrees. Conſequently the force in (B) thus communicated by the Stroke will be as its Square, or 16.

2. Let (B) move forward with two degrees of Velocity, and (A) follow it with 10 degrees; the reſpective Velocity will be 8 as before; conſequently by Paragraph 10th of this Paper, the Strokes in both caſes are equal. The Velocity in (B) after the Stroke will be half the Sum of the Velocities before the Stroke, or 6 degrees, by Paragraph 8th.

According to the new Opinion, the Forces being as the Squares of the Velocities, the Force of (B) before the Stroke will be to its Force after the Stroke, as the Square of 2 is to the Square of 6; *i. e.* as 4 is to 36. Subduct the Force in (B) before the Stroke, from the Force it has after the Stroke, and you have the Degrees of Force communicated by the Stroke: Which, if this Opinion were true, would be 32, *i. e.* juſt double the Number of Degrees communicated by the ſame Force in the former Inſtance, which was but as 16. Thus equal Strokes produce unequal Effects in our ſenſe of Effects.

14. The following Table gives ſeveral other Inſtances. In the three firſt Columns you have the Velocities of the two Bodies both before, and after the Stroke; in the two next, you have the Forces in (B) both before, and after the Stroke; and in the ſixth, the Difference of thoſe Forces, or the different Degrees of Force effected by the ſame Stroke; and in the laſt Column, the Proportion of thoſe Forces, or Effects of the Cauſe or Stroke.

The

The Velocity in			The Forces in B.		Force communicated by the Stroke.	Proportion.
A	B	B				
8	0	4	0	16	16	1
10	2	6	4	36	32	2
14	6	10	36	100	64	4
18	10	14	100	196	96	6
22	14	18	196	324	128	8
26	18	22	324	484	160	10
Before the Stroke.		After the Stroke.	Before the Stroke.	After the Stroke.		

15. If it be ſaid, that I have not conſider'd the other part of the entire Effect of the Stroke, the Intropreſſion of the Parts; I reply, this will make but a ſmall Alteration in the matter; ſince the Intropreſſions in all theſe Caſes are equal, (Paragr. 11.) the relative Velocities being by ſuppoſition the ſame: So that notwithſtanding, upon the whole, one and the ſame, or equal Cauſes, will produce unequal Effects.

VII. *Remarks upon a ſuppoſed Demonſtration, that the moving Forces of the ſame Body are not as the Velocities, but as the Squares of the Velocities. By the ſame.*

THE Demonſtration runs thus: "Concipio cor-
" pus C *Fig.*7. moveri obliquè in elaſtrum L, ve-
" locitate C L ut 2, angulo inclinationis C L P exiſtente
" 30 gr. cujus nempe ſinus C P eſt ſemiſſis radii C L.
" Suppono autem eam eſſe reſiſtentiam in elaſtro, ut
" ad illud tendendum requiratur præcisè unus velocita-
" tis gradus in illo corpore, ſi perpendiculariter im-
" pingeret. Quid ergo jam fiet poſt incurſionem ob-
" liquam corporis C in elaſtrum L? Quoniam motus
" per C L componitur, ut notum eſt, ex duobus colla-
" teralibus per C P & P L, & cum C P, ſecundum
" quam corpus directè impingit in elaſtrum L, expri-
" mit dimidiam celeritatem corporis per C L, conſume-
" tur hic motus per C P, tenſo elaſtro (perinde enim eſ-
" ſet, ac ſi corpus C celeritate C P perpendiculariter
" incurreret in elaſtrum, quod per Hypotheſin, eam
" celeritatem deſtruere poſſet) remanente corporis ce-
" leritate, & directione P L. Producta igitur P L in
" M, ita ut L M ſit = P L = $\sqrt{3}$ (ponitur enim C L
" = 2) & applicato in M alio ſimili elaſtro faciente
" cum L M angulum L M Q, cujus ſinus L Q = C P =
" 1, per eandem rationem manifeſtum eſt corpus C,
" poſt tenſionem elaſtri L, tenſurum eſſe elaſtrum M,
" amiſſo motu per L Q, & ſervato motu per Q M.
" Prolongata itaque Q M ad N, ut fiat M N = Q M
=

" $= \sqrt{2}$ ibique ſubſtituto elaſtro ſimili tertio conſtitu-
" ente cum MN angulum MNR ſemirectum, quo
" ſcilicet MR iterum ſit $= CP = 1$; patet ſimiliter
" motum per MR totum impendi in tenſionem elaſtri
" N, corpore interim moveri pergente directione &
" celeritate RN = 1. Denique ſi hac celeritate reſidua
" impingat perpendiculariter in elaſtrum O, huic ten-
" dendo totam ſuam vim reliquam dabit; ipſum ita-
" que corpus ad quietem redigetur. Hiſce ita præ-
" miſſis, patet nunc potentiam corporis C tantam fu-
" iſſe, ut per ſe ſolum tendere poſſit præcisè quatuor e-
" laſtra talia, ad quæ ſingula ſeorſim tendenda requi-
" ritur dimidia velocitas corporis æqualis ipſi C, ade-
" óque cum effectus illius quadruplo major ſit quàm
" effectus hujus, evidens eſt quoque vim corporis velo-
" citate 2 grad. quadruplam eſſe vis corporis ejuſdem,
" vel æqualis, velocitate 1 grad.

" Haud abſimili modo demonſtrarem corpus C ve-
" locitate 3 grad tendere poſſe 9 elaſtra, ad quorum u-
" num tendendum unus velocitatis gradus in eo cor-
" pore requiritur, & tandem in genere numerum ela-
" ſtrorum tenſorum ſemper eſſe quadratum numeri
" graduum velocitatis. Unde igitur ſequetur, vires
" corporum æqualium eſſe in duplicata ratione celeri-
" tatum. Q E D.

1. This Argument is ſounded entirely on the commonly received Doctrine of the Compoſition and Reſolution of Forces, and not upon any deciſive Experiments, that have been actually made upon this occaſion.

2. All that is proved from this Doctrine, is, that a Body moving with two Degrees of Velocity, may be made to bend 4; with 3 Degrees of Velocity it may be made to bend 9 ſimilar Springs, each deſtroying one

Degree

Degree of Velocity in a perpendicular Direction, before its force is entirely ſpent, provided you take care to alter the Directions of the Motion in every Stroke but the laſt, after a certain manner: That had the ſame Body moved but with one degree of Velocity in one Direction, and that in a perpendicular one, it would have loſt all its force at once, and bent but one of thoſe Springs: Which is far from proving the thing in Queſtion.

3. To make the Reaſoning on this Head concluſive, the two Bodies ſhould not only be equal in Quantity of Matter, but alike in that material Circumſtance the Direction of their Motions; ſo that if one of the Bodies move in a perpendicular Direction, the other ſhould do ſo too; or if the one ſtrikes in an oblique Direction, the other ſhould do the ſame, and that in the ſame degree of Obliquity; and laſtly, if one moves in ſeveral Directions, the other ſhould do the ſame. But in the caſe before us one is ſuppoſed to move but in one Direction perpendicularly, and the other to move in three oblique Directions, and but one perpendicular.

4. Let therefore the ſame Body move always in the ſame Directions, and with a ſmall Alteration, the Argument uſed in this Demonſtration will be ſo far from proving that ſide only of the Queſtion for which it was brought, that it will equally ſerve to prove the truth of the other, namely, that the Forces of the ſame Body moving with different Velocities are as thoſe Velocities.

Let therefore the ſame Body, inſtead of moving with two Degrees of Velocity, move but with one, and in the ſame Directions as above; only let the Springs be capable of deſtroying but half a Degree of Velocity in a perpendicular Direction; then by the ſame ſteps

of

of reaſoning it will follow, that this Body will now alſo bend 4 ſimilar Springs, before its Force is ſpent ; ſo that the ſame Body moving with half the Velocities, and in the ſame Directions as before, bends the ſame number of Springs ; only now the Springs make but half the Reſiſtance, that the Springs in the former caſe made ; therefore the Effect in this caſe, according to our way of eſtimating an Effect, is but half the former Effect ; conſequently the Forces producing theſe Effects are as 2 to 1 ; But in this *Ratio* are the Velocities, with which the Body moved in the two Caſes ; therefore the Forces are as the Velocities.

Let the Body move with 3 degrees of Velocity, and it will bend 9 ſimilar Springs, each deſtroying one Degree of Velocity in a perpendicular Direction, before the whole Force is conſumed. So alſo by the ſame way of arguing, 'tis as certain, that if the ſame Body move with one degree of Velocity, it will bend 9 ſimilar Springs, each deſtroying a third part of one Degree of Velocity in a perpendicular Direction, before its Force is extinguiſhed : So that ſtill the Effects, or Reſiſtances overcome in the ſame Directions, are, according to our way of computing, as 3 to 1 ; and ſo alſo their Forces muſt be but in the ſame *Ratio* of 3 to 1, as were the Velocities ; conſequently the Forces are as the Velocities.

5. Since therefore this Proof drawn from the Doctrine of Compoſition and Reſolution of Forces equally proves both ſides of the Queſtion, it proves too much, or in reality nothing at all ; and is therefore far from deſerving the Name of a Demonſtration.

VIII. *Bre-*

VIII. *Brevis Commentatio de Cobalto, Auctore Viro Clariſſimo,* Joh. Henr. Linckio, Lipſienſi, *Acad.* Cæſar. Leopold. Carolin. *Nat. Curioſ. & Soc. Reg.* Anglic. *Sodale.*

INter plurima Mineralium genera, vix datur aliud, quod tanta nominum farragine, æquivocatione, & confuſione intricatum, tam operosâ explicatione indigeat quam Cobaltum. Veteres de Cadmia & Marcaſita quædam, quaſi edentuli, murmurant; poſterioris ævi Viri de Auripigmento, Sandaraca, Tutia, Spodio, Pompholyge huc miſcuerunt ſermones; tandem Lapide calaminari, Zinco, Cobalto, auribus obſtrepitum noſtris eſt; & ſub Cobalto denique ſunt, qui jam vaporem metallicum, jam pygmæum ſubterraneum ſubintelligunt. Quanqnam enim ejuſmodi tituli res innuant conſanguinitate pariter & uſu non parum conjunctas; ſubjectis tamen & characteribus fere omnes, & uſibus deſtinatis non paucas ab invicem differre, maxime tenendum eſt. Jam quidem de eo ſaltem, quod hodie, noſtris inprimis, Cobaltum appellatur, agere apud animum meum propoſui, atque dicam, ſi non plene, plane tamen, quod res ipſa ſit, non curatis denique de verbis, quæ moveri poſſent, diſputationibus. Cum vero periculum ſit, ut omnes, imprimis per libros, & ſine igne Philoſophi, omnia, uti decet, intelligant, atque evolvendo naturalium rerum homonymias, ac titulorum ſignificatus proprios, analogos, æquivocoſque, non parum lucis adferamus legentibus, non ſuperfluum fore duxi, de illis quædam præmittere.

Cad-

Cadmia, Καδμεία, ut antiquiſſimum, ita obſcuriſſimum nomen Græcorum eſt, propriè Thebarum arcem, Cadmeam, à Cadmo, Agenoris filio, in Bœotiâ conditam; impropriè verò jam Lapidis Philoſophorum materiam, vel ſi velis, illum ipſum ſignificans; (unde adopturientes, vel adepti, ſocii Cadmi audiunt, h. e. viri, quibus arcem naturæ, plurimis inexpugnabilem, occupare contigit) jam mineram aliam, qualis Lapis Calaminaris eſt; jam ſublimatum quoddam in tingendo ad flavum cupro cum iſto lapide coincidens, denotat; unde Cadmia foſſilis, Cadmia fornacum, Cadmia metallica; porrò Cadmia atramentoſa, *i. e.* ea, quæ vel per ſe atramentum, ſeu vitriolum, qualis Lapis Calaminaris eſt, vel ex accidenti, *h. e.* ab admixta minera pyritoſa exporrigit. Quod ſi igitur à quopiam Cobaltum hodiernum Cadmiam appellatum audiveris, vel ab ipſo ita appellari alicui libuerit, faciat; evitet modo incongruam vocis acceptionem. Quod tutius fieri non poterit, quam ſi pronunciaverit nomen cum addito, Cadmia pro Cœruleo, de qua hic ſermo eſt. Marcaſita, originis Arabicæ vocabulum,

I. Apud Metallicos denotat pyritam ſubflavum, angularem, in ſpecie hexaedron, ſeu cubicum, politum, cæterùm ab alio, quoad eſſentiam & cætera planè non differentem, atque ita potiſſimum ſulphur & ferrum, adhæc fortè cupri arſenicique particulam ſæpe continentem; auri itaque ac ejuſmodi ſpecioſos titulos promerentem minime;

II. Apud Mercatores, quos Materialiſtas vocant, pariter & apud Pharmacopolas famoſum illud ſemimetallum, Biſmuthum;

III. Apud Alchymiſtas, vel materiam primam, vel ſecundam, mineralem, crudam aliquam, eamque ſemimetallicam ſeu mercurialem, qualis dictum Biſmuthum, regulus ♂, Zincum & o=o inprimis ſunt. Quatenus

tenus igitur non modo arſenicum Cobalti, ſtrictè ſic dicti, pars eſſentialis eſt, ſed & Biſmuthi minera cum dicto Cobalto intimæ cognationis vinculo tenetur, atque illud, idque verè egregius cœruleum, quam Cobaltum, ſiſtit vitrum, eatenus Marcaſitæ appellatio hic locum aliquem invenit; dummodo evitetur falſa vocis acceptio, ac intelligatur, eam anguſtam nimis & adæquatam minus eſſe, nempe Cobaltum plus quam Marcaſitam, *h. e.* non ſolùm ſubſtantiam ſemimetallicam illam, Biſmuthum, & aliam ſcilicet arſenicalem, ſed etiam terram in cœruleum vitreſcibilem, in ſe comprehendere.

Auripigmentum noſtris equidem temporibus nihil aliud eſt, quàm minera illa lutea foliacea, arſenicali-ſulphurea, *Operment.* Sunt verò, qui latiori illud acceperunt, *h. e.* non ſolùm materiali, ſed etiam effectivo ſignificatu. Neque hoc quidem ſine omni cauſa. Auripigmentum enim, præterquam quod aureum præ ſe ferat, picturiſque inferat colorem, & in habitu ad metalla ejuſmodi ſubjectum non incongruè denotat, quod colore luteo cuprum imbuit, quale inprimis Zincum, Cadmia fornacum, Cobalti non diſſita agnata ſunt. Ubi verò negari non poteſt, Cobaltum noſtrum neque ratione partis volatilis, ſemimetallico-arſenicalis, ex qua ad cuprum dealbationis potiùs gaudet energia; neque ex cauſa terræ vitreſcibilis hunc in finem ullatenus convenire. Ab arſenico tamen, ſive album, ſive flavum, ſive rubrum ſeu Sandaraca, eaque tam foſſilis, quam factitia ſit, non rarò cognominatur id, quod à Cobalto rectius fieret; dicentibus noſtris Freibergenſibus, tam metallicis, *v. g.* hanc vel illam mineram Cobalticam eſſe, ſeu *Kobold*, quam fuſoribus, quando jam fumos, jam quædam in furnis & toſtrinis ſublimata appellitant cobaltica, & nobili metallo deletoria:

(*Cobal-*

(*Cobaldiſch und rauberiſch :*) Atque hoc Cobaltum putatitium non niſi pyrites ſulphureo-arſenicalis ibi eſt, quum tamen verum ab omni ſulphure planè liberum ſit, & pyrites talis terra pro cœruleo penitus deſtituatur. Quin Freibergæ Cobaltum ferè incognitum, minimum reperiendum verum vix eſt, ſed Schneebergenſium & Annæbergenſium fines adeundi. Proinde male conſules receptis & laboribus tuis, ſi Cobaltum promiſcuè expetieris, hanc vel illam abuſivam vocis acceptionem, quæ ſingulis interdum ſemimilliaribus alia deprehenditur, non cogniturus.

De vapore metallico & Pygmæo non eſt, ut pro avertenda æquivocatione hic adeo ſimus ſolliciti ; ſunt tamen fumicapii & ventivenduli, qui in ejuſmodi materia vaporoſa ſemen metallorum, materiam & mercurium Philoſophorum quærendum volunt : & nota metallicorum hic dictio eſt: *Der Schwaden, der Kobold ſey aufgeſtiegen, wovon man habe aufreiſſen müſſen, auch mancher Bergmannerſtioket worden*; i. e. Cobaltum ſeu vaporem metallicum aſcendiſſe, & hunc operarium ſuffocaſſe. Quem dicendi morem credo matrem iſtius opinionis maximam partem eſſe, qua creaturæ animatæ ſubterraneæ, *Erd-und Berg-geiſter, kleine Koboldgen* finguntur. Squidem verba, prout jacent, non aliter ſonant, ac ſi agens quoddam intellectuale ſuffocationem & mortem hominis intenderit, & manibus quaſi perpetraverit. Non aliter atque cum Paracelſi ſpiritibus, homunculis cucurbitalibus, atque ejuſmodi phænomenis, ſuo tempore ſtupendis, comparatum fuiſſe puto, quando jam album, jam rubrum, jam viridem ſpiritum ex Endore ſuo, *h. e.* per alembicum, aſcendere & provenire ait, atque adhuc tyrones noſtri ſine omni incantatione producere non neſciunt.

Ex omibus hiſce Cobalti & Cadmiæ nomina ad rem præſentem conveniunt, & ſi hæc rectè conſideretur, pro Synonymis haberi debent. Quoniam autem Cadmiæ vocabulum apud ſcriptores plurimis laborat dubiis, hoc miſſo, Cobalti titulum ſervamus, vocabularium metallicorum ſequentes, quicquid ſcholaſticorum turba contra literarum barbariem obganniat. Cæterùm de hiſce nominibus & rebus meretur conferri Henkelii, qui Freibergæ in Hermunduris metalli feraciſſimæ, ut & agrorum & metallifodinarum adjacentium Phyſicus eſt experientiâ celebris, & ſocietatis Berolin. Membrum eſt, *Pyritologia*, cap. 2. Cobaltum igitur eſt minera griſea, obſcurè albicans, mineræ argenti albæ, & pyritæ albi, quoad colorem æmula, ſed paulò obſcurior, continens arſenicum album & terram fixam, cum ſilice & cineribus clavellatis in vitrum cœruleum abiens.

Effoditur in Schneebergenſium & Annæbergenſium terris, inprimis in Cervi, Rappolti, centum millia equitum, Galilææ, & quotquot ſunt, puteis; & quidem in venis propriis, idque ita purum putumque cum duriſſimo ſilice, ſeu, quod vocant quarzo, ut, ſi quæ fortè alia minerarum genera interſperſa ſeu adfixa alicubi reperiantur, non ſimul cum vera Cobalti ſubſtantia, ſilici dicto innata, ſed aliis ex cauſis, imo aliis venis aſſociata non obſcurè appareant.

Signorum externorum ejus proximum & indubium eſt minera quædam roſei coloris, ſtructuræ radiatæ, quam Cobalti florem, *Cobald-Blüthe* vocant; inter egregia & ſpectatu digna in minerarum gazophylaceis recondita. Hujus operis datur & alia, coloris verò non adeo roſei nitidi, ſed pallidioris, nulliuſque ſtructuræ, ſed pulveris inſtar mineram ambientis.

Signum

Signum propius præbet accurata ocularis collatio mineræ hujus cum pyrita albo; accuratior cum minera cupri grisea; accuratissima cum minera argenti alba; cum quibus rebus intersest ipsi non levis coloris similitudo, essentiæ maxima differentia; quod verò verbis nec exprimi, nec concipi satis potest, ita, ut frequenti opus sit intuitu. Vid. laudata *Pyritologia*, p. 193, sq.

Signum proximum, seu res ipsa manifestatur ex vitro inde confecto, quod ex pyrita nigrum, ex dicta cupri minera rusum, ex argenti alba, & cupri participe minera, jam magis minusque nigricans; ex Cobalto vero sapphirinum prodit, unde & Zaffera, tum pro Cobalto tosto, tum etiam pro vitro cœruleo in pulverem redacto, nomen ortum, verosimile est.

I. Per se Cobaltum auræ, pluviæ, soli in cumulo diu expositum efflorescit rosei vel persici coloris, qui dicitur, flos Cadmiæ, sed cum illo flore nitidiori antea descripto, non confundendus. Ille ritè extractus; reddit substantiam elegantissime rubram, & ad hæe vitrioli obscurè viridis portiunculam, quæ merentur ulteriorem investigationem; mineræ autem non parvam quantitatem, tempus & assiduitatem & patientiam requirunt, cujus rei encheireses noster laudatas D. Henkel compertas probe habet.

II. Spiritus Nitri solvit illud cum impetu & effervescentia, *v.g.* vi. vel iv. partes ad unam, quatenus minera pura vel minus est; unde verò resultare solet solutio non unius coloris. Viridis nempe color Vitrioli Veneris prope est, si cuprum certè continet; quod apparet in illa Cobalti minera, quæ *Kupfer-Nikel* appellatur, & adhuc cruda cupreum colorem præ se fert. Viridis tamen etiam provenit aliquando ex ea minera, quæ tota grisea, & nullatenus ad visum cuprea, quamvis

vis ad colorem ſolutionis Vitrioli Martis puri accedens; tamen à cupri contactu non planè aliena. Obſcurè flaveſcentem ſolutionem reperio optimam ad vitrum illud ſiſtendum cœruleum. Rubicunda implicatam ſibi ſignificat mineram Biſmuthi.

III. Solutiones, cum Alcali, *v. g.* oleo tartari per deliquium præcipitatæ reddunt pulverem porcellanæ debitè applicatum, egregiè cœruleum, ſi contigit, cætera paria, inprimis Cobaltum ſelectiſſimum, atque ignis te eſſe expertum directorem.

IV. Acidum Vitrioli & Salis communis adoriuntur quidem Cadmiam noſtram, ſed non tam ſolvunt, quam erodunt, inprimis Vitrioli acidum, & in pulverem album reducunt, adeò ut ſpiritus Nitri convenientior ſit ſolvendæ Cadmiæ.

V. In vaſe clauſo, retorta lapidea loricata, vel ſublimatorio alio, igne aperto debitè detentum effumare patitur partem volatilem arſenicalem, ab initio aliquantulum fuliginoſam, imo & ſandaraceam, atque ita ſulphuris communis non planè expertem, quam maximè autem ſe colligentem in forma ſemimetallica ſplendida, regulina, foliata, qualis ex pyrite albo provenit, ubi verò ſplendor per dies, imo per horas evaneſcit, luculento indicio, particulas aërias in ſubſtantiam illam habere ingreſſum.

VI. In furno vero toſtorio (*Roſt-olfen*) ubi flammæ igneæ mineram contingunt, adhæreſcit camino vel ductibus adſtructis, ſicuti pulvis (*Mehl*) cinereo-albicantis coloris, qui colligitur pro conficiendo illo arſenico albo cryſtallino.

VII. Re-

VII. Remanet terra fixa, cinerea, vitreſcibilis.

VIII. Minera Biſmuthi (de qua quidem hoc loco ſpecialiſſimus ſermo non eſt, tamen cœruleo maximè inſerviens, non prætereunda) igne aperto facilè effluere ſinit ſubſtantiam ſemimetallicam, quæ Biſmuthum appellatur, & ad alios uſus venditur; & relinquit lapidem ſeu terram itidem griſeam, fixam, dictam *Biſmuth-graupen*.

IX. Minera illa quantum poſſibile, ſi pura, ſegregatur ex Cobalto ſtrictè ſic dicto, ut Biſmuthum colligatur. Sed interdum, imo non raro illud ita inſperſum & intertextum eſt, ut ſeparari ab hac minus planè queat; hinc in ollis vitrificatoriis ſubſidet ſubſtantia regulina, dicta *Speiſſe*, coloris communiter albo-ſubrubicundi; quæ tamen rarò vel nunquam Biſmuthum ita purum eſt, ſicuti hoc ex ſua minera per ſe emanat, ſed commixtam ſibi habet peregrinam inter alia terram fixam cobalticam. Atque ita novis compoſitionibus vitrificatoriis iterum commiſcetur. Subdividunt aliqui regulum hunc in Marcaſitam, *Glokenſpeiſſe*, & æs caldarium, quorum illa inferius, hoc ſuperius hæret.

X. Terra utrinque remanens fixa, per ſe difficillimè vel planè non in vitrum abit, quamvis in furno vere vitrificatorio per 8 horas illam detinuerim, vix quod unam vel alteram maculam ſubcœruleam crucibulo Haſſiaco adnotaverit. Et quoniam ſabuli granula argillæ vaſorum iſtorum inhærentia concurrunt, dubium adhuc manet, utrum per ſe talis ſit, quæ ſine addito cœruleum poſſit conſtituere vitrum; quin hoc negandum mihi inde videtur, quia maſſa, ubi vas non tangit, non niſi griſea eſt & manet.

XI. Terra

XI. Terra iſta non planè ſterilis, ſed metalli particeps eſt. Quod cuprum hoc ſit, facilè præſumitur, ſi non è colore ſolutionum, tamen ex vitro cœruleo planè inde confecto, & ex agnata Cobalti minera, *Kupfernikel*. Quod verò pauciſſima metalli qualitas hic ſit, non ſolum ex levitate terræ, ſed etiam exinde appareat; quia neque ex Cadmia optima, neque ex agnata, utut à Venere denominata, ſubſtantia metallica cuprea ullatenus elici & colligi poteſt. Quicquid autem de cupri portiuncula prætendi queat, illa tamen per ſe, quæ ſit, definienda non eſt, niſi quod dicamus, eam Cadmiæ peculiarem eſſe atque ſpecificam.

XII. Datur Cobaltum, quod etiam intoſtum, vitrum cœruleum porrigit, imo quandoque elegantius, quam ubi toſtum fuit; quod non ex aſpectu mineræ, ſed per experimentum diſcendum eſt.

XIII. Itidemque per experientiam eluceſcit cognitio circa ignis gradus & continuationem, ſiquidem alia minera longiore & breviore, fortiore & leviore opus habet toſtione.

XIV. Adjuvatur & vitrificatio & coloris elegantia per additum pulverem arſenicalem, imo arſenicum cryſtallinum ipſum.

XV. Regulæ quidem non planè non univerſales proſtant, ad quas Cobalti tractatio pro cœruleo vitro inſtituenda eſt; ſed non conſtantes, quod jam ſignificavi Opus igitur eſt non ſolum in privato laboratorio variis, iiſque tam ſecundum proportiones, quam ignis modos & gradus, & reliqua, experimentis; ſed & in officinis Cobalti ipſis, ut docimaſta indefeſſe experiendo invigilet naturæ mineræ & affixarum, atque

ita

Numb. 397.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *January*, *February*, and *March*. 1727.

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Jos: Banks

ita compoſitionibus & laboribus, utpote in re colorum quaſi delicatiſſimis, rectè proſpiciat, iiſque ad votum minus ſuccedentibus vel addere, vel abſumere, vel corrigere aliter ſciat.

XVI. Cobaltum igitur in hunc finem experturus, probe oculis conſideret illud, utrum & quam purum à lapide & adhærentibus, vel hoc minus aut quam minus ſit; torre vel calcina, donec fumi ceſſent; commiſce pulveri remanenti ſilicis vel ſabuli ſilicei puriſſimi, albiſſimi, 1, 2, vel 3 partes, cinerum clavellatorum vel partem, vel pro fluxilitatis mineræ gradu plures, miſce probe ingere in crucibulum, ſi datur, ſubtus conicum, quali utuntur cuprum experientes, *Kupfer-Tiegel.* Si bene fluxerit, quod baculo ferreo experieris, quodque ocius ſeriuſque fit, quatenus furnus & ignis ſunt, exime crucibulum, & frigefactum vitrum ſcoriis ſepara; ſeparatum pulveriſa, non in vaſe ferreo, utpote colorem obſcurante, ſed, ſi proſtat, in porphyrio, vel alio duriſſimo lapide; pulverem lava ab eo, quod albicantis ſive griſei coloris eſt, *Eſchel* ſeu *Schlamm* vocatum, atque tum hoc, tum illud deſicca; deſiccatum utrumque trajice per cribrum anguſtiſſimum; quod craſſum, iterum tere cum aqua, deſicca, donec omne comminutum ſit. Tandem confer cœruleum obtentum tuum cum exemplaribus cololorum, (*Muſter,*) quæ gradus, ordinem, titulos atque pretium ſuum publica lege jam conſtituta habent, & videbis, quid Cobaltum ſumptum importet, & ubi error aliquis factus ſit.

XVII. In groſſo, ut aiunt, procedendi hic modus eſt. Cobalti minera è puteo tracta, ab heterogeneis omnibus malleo, quantum poteſt, ſeparata, calcinatur in furno, qui fornicatus, latus, plani ferè fundi eſt, atque ignem flammeum ab alio prope adſtructo furno anemio ſuper mineram recipit. Huic ex oppoſito furni

anemii introitus apertus eſt, ubi quidam rutro Cobalti terram circumvolvit, quo ignis undiquaque in eam agat, atque arſenicum eò melius disjiciat, donec non amplius fumet. Adhæc calcinantur ſilices electi puriſſimi, & jam candentes projiciuntur in aquam frigidam, ut magis tractabiles fiant; ita præparati tunduntur in pulverem ſeu ſabulum. Tum Cobalti toſti pars una cum ſilicum partibus ut plurimum tribus, & ut eo facilius fluat, cinerum clavellatorum parte una, in ciſta quadam commixta funditur in furno vitrificatorio, in magnis ollis, igne per 8, 10, imo 12 horas intenſiſſimo; atque operarius (*der Schurknecht*) unco ferreo magma jam fluens ſtrenuè circumagitet, ut omne in æqualem fluxionem abeat. Ubi tandem, quam poteſt limpidiſſimè fluit, cochleari magno ferreo vitrum exhauritur, & in alvearium, aquâ repletum, projicitur. Vitrum ita magis friabile redditum tunditur malleo, quem aqua agit (*gepucht*;) tuſum trajicitur per cribrum orichalceum. Remanens ulterius tunditur, & trajectum in mola cum aqua ſubtiliſſimè pulveriſatur. Hoc lavatur tum ad abſterſionem particularum ſalinarum, aliarumque ſordium, tum ad ſeparationem pulveris illius cinerei albicantis levioris, quem *Eſchel* appellant, à cœruleo, quod *Blaue Farbe* dicitur, ſecundum rationem, quod aliàs *Schlemmen* dicunt. Tandem in vaſa per centenaria conditur, & per totum terrarum orbem divenditur. Sicut verò hujus rei, atque hinc pretii maxima differentia eſt, ita vaſa certis litteris notantur, ita ut O.C. ſignificet *Ordinair Cobolt*. M. C. *Mittel Cobolt*. F. C. *Fein Cobolt*. F.F.C. *Fein, Fein Cobolt*. F.F.F.C. *Fein, Fein, Fein Cobolt*; id quod vero ut pretioſiſſimum, ita & rariſſimum eſt. Minera Cobalti ita præparata, aliàs Schmaltum, *Smalte*, & mulieribus, *blaue Stärcke*, quo utuntur pro linteis dealbandis, communiter appellatur.

XVIII. Ven-

XVIII. Venditur & Cobaltum tantum toſtum,& cum duabus vel tribus partibus ſilicum vel quarzi commixtum, ſub titulo *Zaflor*, item *Zaffera*, pro figulis. Ubi hoc meretur commemorari, quod pulvis hicce in vaſis, quibus devehitur, tam compactum fiat, & quaſi concreſcat, ut duri lapidis inſtar, malleo opus habeat ad comminuendum.

XIX. Cæterùm Cobaltum per ſe argentum non continet. Quamvis enim detur ejuſmodi vena, ex qua nobile hoc metallum excoquatur, tamen non niſi per accidens fieri exinde liquet, quia eadem vena illic plus, hic minus, & ut plurimum planè non argentifera eſt.

XX. Hoc non obſtante, argentum nativum ſuper Cobaltum non rarò reperitur, quod verò non aliud, quam hoc arguit, mineram illam argenti non incongruam eſſe matricem.

Tantùm ferè eſt, quod, propriâ experientiâ de Cobalto cognitum, recenſione dignum duxi. Quod ſupereſt, L. B. rogo, hanc rudi, quod dicunt, Minervâ conſcriptam commentationem, ſereno vultu velis ſuſcipere, & cogitare, quod nec ego iſtud abs me alienum arbitrer, quicquid iis ſolet accidere, qui, nullo duce prævio, fragoſas & inacceſſas vias, ad Naturæ arcem tendentes, primi invadunt. Vale. Perſcripſi *Lipſiæ*, Menſe *Quintil.* A. R. S. MDCCXXVI.

FINIS.

ERRAT. Pag. 197. Lin. 24. lege *laudatus.*

I. *Remarks upon ſome Diſſertations lately publiſh'd at* Paris, *by the Rev.* P. Souciet, *againſt Sir* Iſaac Newton's *Chronology.* *By Dr.* Edmund Halley, *Aſtronomer-Royal, F. R. S.*

THere has been lately put into my hands a Book publiſhed laſt Year at *Paris* by Father *Souciet*, Jeſuit, againſt our late Preſident Sir *Iſaac Newton* his *Chronology*, without waiting till the Book be publiſhed, and without knowing the Contents thereof, otherwiſe than by a ſhort Extract, made at the deſire of a very great Perſon, and without intention that it ſhould be publickly ſeen. However, a Copy thereof having been (as I ſuppoſe) ſurreptitiouſly obtained and carried over into *France*, the ſame was firſt tranſlated into *French*, and then printed at *Paris* with a pretended Refutation thereof by the ſame *P. Souciet*. Since then, Sir *Iſaac* having anſwered, as he thought, his Objections, has thereby given him a handle to publiſh five other Diſſertations againſt the new Syſtem of Chronology, as he calls it; the firſt and laſt of which, being chiefly Aſtronomical (ſince the great Author is no more) ſeem properly to fall under my examination, both on account of the Poſt in which I have the honour to ſerve his Majeſty in quality of his Aſtronomer, as alſo from the long Acquaintance and Friendſhip that has ſubſiſted between the Deceaſed and my ſelf.

And firſt, I obſerve, that *P. Souciet* readily allows what ſeems to be the moſt exceptionable part of the whole Syſtem, *viz.* that *Chiron* the Centaur fixt the

Colures, in the ancient Sphere of fixt Stars, in the ſame places as *Hipparchus* tells us they had been ſuppoſed by *Eudoxus* many Centuries of Years after *Chiron*. His words are theſe, ἐν δὲ τῷ ἑξῆς πολὺ φησὶ κεῖσθαι τὸν κήτους τὴν κεφαλὴν, καὶ τὸ κεῖσθαι τὰ νῶτα κατὰ πλάτος. This, undoubtedly, was the Poſition of the Colure of the Vernal Equinox many Ages before *Eudoxus*; but whether ſo old as *Chiron*, and the *Argonautick* Expedition, I ſhall not undertake at this time to enquire; but only obſerve, that *P. Souciet* in his *Faſtes du Monde*, or Abridgment of his Chronology, prefixt to theſe Diſſertations, makes the *Argonautick* Expedition 1467 Years before our *Æra* of the Birth of Jeſus Chriſt; and the taking of *Troy* 1388 Years before it, which Date is 120 Years ſooner than the *Parian* Chronicle, read and publiſht by our Learned *Selden* in his *Marmora Arundeliana*, makes it; and above 500 Years earlier than the time aſſigned by Sir *Iſaac Newton*.

Now both of them making uſe of the ſame Premiſſes, it may ſeem ſtrange that their Concluſions ſhould be ſo widely diſtant: And indeed upon a Præpoſſeſſion that the *Argonautick* Expedition and the Siege of *Troy* could not have been leſs than 1000 Years before Chriſt, I muſt own, I was at firſt ſomewhat prejudiced in favour of *P. Souciet*, taking his Calculations for granted; and not having ſeen Sir *Iſaac*'s Work. But obſerving that he quotes Sir *Iſaac*, as ſaying, that in conſequence of what *Hipparchus* has recorded from *Eudoxus*, the Equinoctial Colure in the old Sphere was about 7° $36'$ from the firſt Star of *Aries*, I was reſolved to examine the matter with due Attention, eſpecially ſince the Good Father ſeems to triumph over his Adverſary, and to treat a Man of his Figure in the Common-wealth of Learning in a very ludicrous manner, notwithſtanding the ſeveral fine things he ſays of him to palliate it.

I find

I find the Diſpute to be chiefly over what part of the Back of *Aries* the Colure paſt: The Words of *Hipparchus*, as from *Eudoxus*, are ſimply, that it paſt over the Back, without ſaying over what Star, or over what part of the Back it paſt. And the ſame *Hipparchus* ſhews, that if it paſt over the Star in the middle of the Back, it greatly differed from the Situation thereof in his time; and conceiving thence that the Æquinoctial Points might have a regreſſive motion, he was the firſt that attempted to define their motion; but having no Obſervations older than thoſe of *Timocharis*, made within leſs than 200 Years of his own time, and very courſe withal, he was not able to determine the quantity thereof, but gueſt it to be about a Degree in 100 Years; which, length of time, and the more curious Obſervation of the Moderns, has now proved to be 1° 24′. or rather 50″ *per Annum*.

In a word, Sir *Iſaac* takes the Colure to have paſt over the middle of the Conſtellation of *Aries*, and very near the Star in the middle of the Back (*ν Bayero*). And *P. Souciet* will have it, that it paſt over the middle of the Sign or *Dodecatemorion* of *Aries*, reckoning the Sign to begin with the firſt Star of the Conſtellation; and by conſequence his Colure muſt paſs about midway between the Rump and firſt of the Tayl of *Aries* (ε and δ *Bayero*;) which Situation could never be ſaid to be over the Back: But whilſt Sir *Iſaac* makes the Colure but 7° 36′ from the firſt Star of *Aries*, which *P. Souciet* makes 15 Degrees from it, the difference 7° 24′ at 50″ *per Annum*, makes 533 Years difference in the reſult.

Let us now examine when the Stars in queſtion did actually paſs under the Colure of the Vernal Æquinox, aſſuming their places as they are in Mr. *Flamſteed*'s *Britiſh* Catalogue, fitted to the beginning of the Year 1690.

He places the firſt Star of *Aries* in 28° 51′ of *Aries* with 7° 9′. North Latitude. And ſuppoſing the Obliquity of the Ecliptick 23° 29′, it will be as Radius to the Tangent of 23° 29′:: ſo the Tangent of 7° 9′ to the Sine of 3° 7′ ½, the difference of Longitude between the Star and the Point in the Ecliptick which paſt under the Colure, at the ſame time with the Star; ſo that this Point was, *Anno* 1690 *ineunte*, in ♈ 25° 43′ 30″, and therefore allowing 50″ *per Annum*, the Star was under the Colure 1852 Years before the *Epocha* of the *Britiſh* Catalogue, that is, 162 Years before our *Æra* of the Nativity of Jeſus Chriſt; in which very Year *Hipparchus* began to obſerve the Æquinoxes recorded by *Ptolemy*, *Lib.* III. *Cap.* 2.

If therefore with Sir *Iſaac* we add 7° 36′ to the Long. of the firſt Star of *Aries*, as it was in 1690, we ſhall have 36° 27′, which the Colure moves in 2624 Years: And deducting 1690 therefrom, we ſhall have 934 Years before Chriſt for the *Argonautick* Expedition. And if to 7° 36′ we add 3° 7′ ½ we ſhall have 10° 43′ ½, that is, 772 Years before the firſt Star of *Aries* paſt the Colure.

Next let us inquire when the Star in the middle of the Back of *Aries* (*ν Bayero*) paſt the Colure. Its Longitude *Anno* 1690 *ineunte*, was 9° 48′ 35″ of *Taurus*, with North Lat. 6° 8′; but by the foregoing Analogy, the Point in the Ecliptick, over which the Colure paſt at the ſame time with it, was 2° 40′ ½ before it, that is, in ♉ 7° 8′. Now 37° 8′ give 2674 Years nearly, or 984 Years before Chriſt, when that Star was under the Æquinoctial Colure, being but half a Century earlier than Sir *Iſaac* places the *Argonautick* Expedition; and ſhews that he took the middle of *Aries* over which the Colure is ſuppoſed to have paſt, to be the middle of the Conſtellation, and not of the *Dodecatemorion*, and in ſo doing,

doing, no doubt, had reaſon to place this Colure 7° 36′′ in conſequence of the firſt Star of *Aries*, inſtead of 8° 17′, as it was when the Star in the middle of the Back of *Aries* was under the Colure.

But if with *P. Souciet*, you make the Colure to interſect the Ecliptick 15 Degrees from the firſt Star of *Aries*, or 43° 51′ from the Æquinoctial Point, as it was *Anno* 1690, we ſhall have the time nearly 1470 Years before Chriſt; but then the Colure will be very far from the middle of the Back of *Aries*, and leave only his Tayl to the Eaſtward, as it leaves the Head of the Whale to the Weſtward, ſo as by no means to agree with the Deſcription we have of it from *Hipparchus*; which it were to be wiſht had been more definitive, and as well circumſtanced as what *Hipparchus* has left us of the Poſition of the Colures in his own time, which upon examination I find to be very conſiſtent, and the Obſervations made with ſufficient care.

Thus I hope, I have ſhewn *P. Souciet*, that there was no Affectation of Myſtery in Sir *Iſaac*'s placing the Colure 7° 36′ from the firſt Star of *Aries*, nor any occaſion to drole as he does Pag. 131, 132. on that account; as alſo that he ought to have deducted 3° 7′½ out of the 15 Degrees he aſſumes for the diſtance of his Colure from the firſt Star of *Aries*, which will bring him 225 Years nearer to Sir *Iſaac Newton*'s time. He is likewiſe entreated, in the next Edition of his Diſſertations to be a little more careful of his Numbers than he has been Pag. 134, 135, and to inform himſelf in the Sphericks, ſo as to give us the Right Aſcenſions of the Stars truly, from their given Longitudes and Latitudes.

Laſtly,

Laſtly, I would inform him, that the Star in the Centaur which *Hipparchus* diſcribes, as being in his time very near the Autumnal Colure, was not ♆ of *Bayer*, but certainly Φ, and that, *anno ineunte* 1690, its Longitude was *Scorpio* 8° 43′ 40″, with South Latitude 27° 59′. But the Colure paſſing through that Star, by the Proportion given above, cuts the Ecliptick 13° 20′ 50″ in Antecedence of the Star, that is in *Libra* 25° 22′ 50″. But 25° 22′ 50″ give 1827 Years: Wherefore the Time this Star was in the Colure was 137 Years before Chriſt, when *Hipparchus* flouriſhed, and might very well obſerve it.

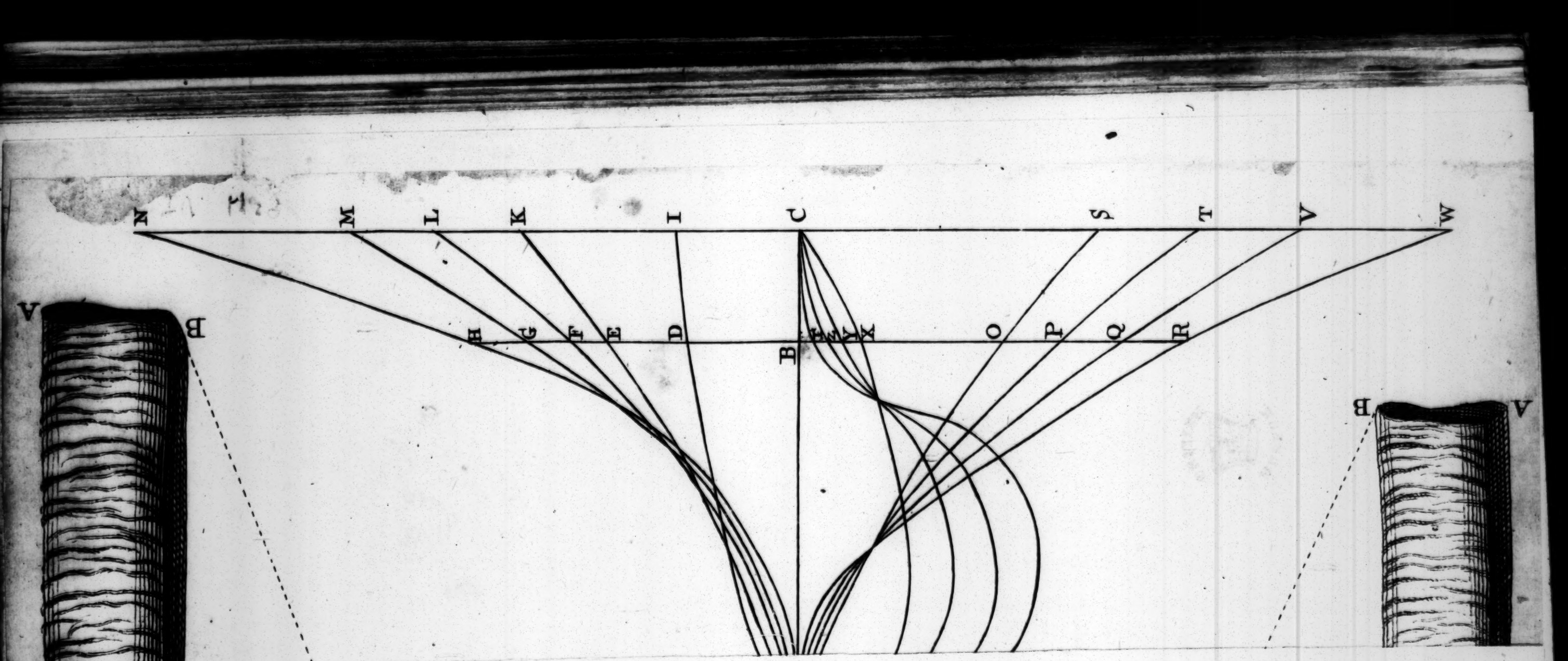
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Fig. 1.

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Fig. 3.

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Fig. 4.

D

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Fig. 2.

B A

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II. *An Account of a large Stone voided through the Urinary Paſſage, by a Woman. Communicated by* Dr. Richard Beard, *F. R. S. Phyſician at* Worceſter.

Worceſter, Jan. 29, 172⅝.

SIR,

AMidſt the variety of Obſervations, that have been made on the extraordinary Powers of Nature, that which follows may deſerve a place. A poor Woman in the Pariſh of *Fladbury* in this County, aged 63, about 3 Years ago, was afflicted with the uſual Symptoms of a Stone in the Kidneys, and afterwards in the Bladder. The Fits of Pain occaſioned by it increas'd as its Bulk did; till ſhe was ſo emaciated by 'em, that her Caſe was judg'd deſperate. Finding relief (towards the end of laſt Summer) by a plentiful uſe of Mallow-Tea, ſhe perſiſted in it for a while; when on a ſudden, in the preſence of ſome Women, ſhe perceived an uncommon weight and force within, which aſſiſting with all the Strength and Breath ſhe had left, a Stone came away with a Noiſe that very much ſurpriz'd the whole Company, and with leſs Pain and Effuſion of Blood then, or Soreneſs afterwards, than might have been expected. She is ſince eaſy and in health, and feels no other Inconvenience now, but that unavoidable one, an *Incontinentia Urinæ*.

The

The Stone (of which I ſend you the ſide-View *Fig.* 1. drawn as near to the Life as we could) is of the ſame Colour and Texture with others of this kind that I have ſeen, formed in humane Bodies. Its weight, as I at preſent find it, is, ℥ ij. ℥ j 55 *gr. Avoird.* When firſt voided, 'twas conſiderably more, ſeveral Pieces having been rubb'd off at *c c*, and likewiſe on the other ſide: The greateſt Circumference is 7 Inches and $\frac{1}{2}$; 'tis 4 Inches $\frac{3}{4}$ round at the thickeſt place, and the length on the Convexity is 4 Inches $\frac{7}{4}$; the parts of the Stone at *a a a* are ſomewhat jagged and ſtain'd with Blood, as is the little Protuberance on the oppoſite ſide *b*. Here I ſuppoſe, it met with the moſt reſiſtance, at the time of its Expulſion thro' the Urinary Paſſage.

III. *An*

III. *Obſervations upon the Comet, that appear'd in* October 1723, *made at* Bombay; *and upon an Eclipſe of the Moon,* Oct. 21, 1724, *at* Gomroon *in* Perſia. *By Mr.* William Saunderſon. *Communicated by Dr.* Halley, Aſtron. Reg. R. S. S.

Bombay, Sept. 12, 1725.

IN the Month of *October* 1723, being riding at *Bombay*, a Brightneſs in the Heavens appear'd in a Right Line, (or but very little to the Eaſtward of one) with *Lyra* and the Bright Star in the *Eagle*, being about 50° diſtant from the laſt; and on *Monday* the *7th* following it had advanced 10° toward the *Eagle*, moving towards it in the forementioned direction, from the S. E. Quarter. I took the following Diſtances between 9 and 10 at Night, as in this Table.

October Days.		Diſt. from the *Eagle's* Heart. °	′	
1	7	40	00	South
2	10	23	50	
3	11	20	30	
4	13	17	40	
5	15	14	40	
6	19	11	40	

At firſt it looked only like one of the White Spots called the *Magellanic Clouds*, the Space filling the Field

of a Six-foot Glaſs. Afterwards I ſaw the Head in the Center of the illuminated Space, which did not look with much Brightneſs; but appeared largeſt on the 10*th* of *October*, decreaſing gradually both in its Bulk and Motion from that time until the 25*th*, at which time I could find no Appearance of it with the fore-mentioned Glaſs. *N. B.* From the 20*th* to the 25*th* it had nearly the ſame Place in the Heavens, ſeeming to move directly from the Earth. *October* the 21*ſt*, 1724, being at *Gomroon* in *Perſia*, the Moon entered into the dark Shadow or *Umbra*, of the Earth at 11′ 33″ paſt 5 A. M.

IV. Ob-

IV. *A Letter from the Reverend Mr.* Edmond Barrel, *Rector of* Sutton *in* Kent, *to Sir* Hans Sloane, *Baronet, Preſident of the* Royal Society *and the College of* Phyſicians, *concerning the Propagation of* Miſſelto.

SIR,

IT hath been my Purpoſe, ever ſince the Death of our late worthy Friend Mr. *Hill*, to communicate to you, (if you would pleaſe to accept of it) an Account of the Growth of *Miſſelto* from the Seed: and I have delayed it from Year to Year, not only to be better aſſured in my Experiments, and to find whether the World had not already, or would not be in the mean Time, better informed of this, by ſome other Hand: But I was alſo in hopes to have been ſo ſucceſsful ſome Year or other, as to have been able, to ſend you and the *Royal Society*, ſome Specimens of the ſeedling Plants, both of the firſt and ſecond Years growth together. I am ſorry that I cannot do this now, and for that reaſon ſhould have delayed this Account, till another Year. But your kind Acceptance of ſo ſmall a Curioſity as I ſent lately, obligeth me to a preſent Compliance with your Requeſt.

The Berries of *Miſſelto* have within their viſcid Pulp a Kernel covered with a thin whitiſh Skin; the inward Subſtance whereof is deeply green, and harder than the Subſtance of a *Piſtachio*-Nut's Kernel. It is flattiſh, and ſhaped ſometimes like a Heart thus ☽; ſometimes oblong thus, O; both are as truly

Seed, as any Plant can have. Thoſe of the oblong Shape put out but one *Germen*; thoſe like an Heart, have two, which prove two diſtinct Plants.

Sir *John Colebatch* recommends the ſowing this Seed by way of Inoculation: Accordingly in *Feb.* 171$\frac{8}{9}$, I endeavoured to place the Berries, within the Bark of *Oak*, *Aſh*, *Beech Pear*, and *Apple*-trees, by making ſeveral Cuts and Gaſhes in the upright Sides of the Trees. The whole Berries would not ſtay in any of them; and when I broke them, the Seed always ſlipped out to the Edge of the Cut, and then it ſtuck to the Bark, by virtue of the ſlimy ſubſtance wherewith it is encompaſſed. I alſo ſtuck one Seed on the bare Bark, without any cutting at all: This ſucceeded beſt, and being the Heart-like ſhape, gave me two Plants. For about the 28*th* of *March* 1719, this, with two more on the *Apple*-tree, and one on the *Pear*-tree, began to ſhoot; and the Growth was in this manner:

The Viſcous Matter having ſtuck the Seed on, and (as it dried) drawn the Seed cloſe and flat down to the Bark of the Tree, there began, in *March* and *April*, to ſpring out of that end of the Seed, which had been toward the Eye of the Berry, a ſmall deep green Shoot or Twigg, very like a ſhort Piece of a little Claſper of the Vine. At firſt, it aroſe upward from the Bark, and then turning again, as it approached the Tree, it ſwelled out ſomewhat bigger round about the End; yet leaving the very Tip or Bottom, quite flat, forming (as it were) a Foot to ſtand upon; not unlike the bottom of ſome Braſs Peſtles. This Foot, when it came to the Bark, which was about *May* or *June* 1719, fixed it ſelf thereon. Being thus faſtned at both Ends, it made a little Arch whoſe Diameter was as long as the Seed, or about $\frac{1}{10}$ of an Inch.

In

In this Condition, it remained all that Year, till about *March* or *April* 1720, and then that part or end of our little Seedling, which was joyned to the Bark, at the place where the Seed firſt ſhot forth, let go its hold, and raiſing it ſelf upward, put forth Leaves, and became the Head of the Plant: and the other end, which ſprung out firſt, and had taken footing in another Place, became the Root of the Plant.

'Tis no uncommon thing, for Seeds of *Ever-greens* to be two Years before they ſpring out of the Ground. And the change of the Ends, firſt one of them ſhooting out, and then the other, was what ſurprized me moſt at firſt; but on further reflexion I found, that Nature, even in this ſtrange Plant, is uniform to her other Productions; in carrying the Sap firſt one way to form the Root, and then turning the Courſe of it back again to ſend out the upper parts of the Plant. The ſtrangeſt and moſt wonderful part is, that the rooting End ſhould make its firſt ſhoot into the open Air, and then turn it ſelf down, to find a proper Place to fix upon. Who could have ſuppoſed, that a Plant, whoſe Berry is the moſt orbicular of any, and therefore the leaſt likely to lie quiet in any Situation, and whoſe proper place of growth is a round and wavering Bough, or upright ſide of a Tree, ſhould after it is once fixed, leave its firſt footing, and ſeek out a new point in the Bark to grow upon.

This is indeed the great Secret of the matter, and ſeems to be the very thing that hath kept the World in Ignorance, about the growing of this Seed. For by requiring a new ſmooth Place of the Bark, whereon to fix the rooting Part, it hath fruſtrated all attempts of ſowing it in the uſual way of other Seeds.

Theo-

Theophraſtus, (about two Thouſand Years ago) ſeems to endeavour at a Reaſon, why this Seed could not grow in the Earth: But all that he, or any one ſince, hath ſaid upon it, is only to agree, that in Fact it doth not, and to wonder why ſo perfect a Seed ſhould not grow in the Earth. That Antient Author rationally concluded, from its having a Seed, that the Plant muſt come from that Seed: Whereas latter Times have been ſo fond of allowing Chance a ſhare in the Productions of Nature, that *Scaliger* hath not only experimentally confuted the common Notion of *Miſſelto*'s being ſown in the Dung of the *Thruſh*; but argueth alſo, very ſtenuouſly againſt the Poſſibility of this Plant's growing from its Seed. Even the great Lord *Bacon*, Sir *Thomas Brown*, *Lobel*, and the inquiſitive Mr. *Ray* (ſo late as 1673,) do all give into it, that this Plant, hath a ſpontaneous and æquivocal, rather than a ſeminal and univocal Generation.

Scaliger's ſtrongeſt Objection is, *Quod è Ramis quibuſdam exit Viſcus, quo in loco nullis modis vel ſtercus conſiſtere, vel ſemen unquam potuerit hærere — Nihilo enim commodius conſiſtere quam in re proclivi Globum.* *Lobel* objects againſt it, becauſe of the Imperfection of the Berry *Acinulo illo pallido pellucido.* Mr. *Ray*'s Argument is, *Viſcus innatus etiam in pronâ Ramorum parte.*

'Tis the property of true Experience to clear up Doubts, and anſwer Objections: And if Nature had been well examined, it would have appeared, that this Seed is of a ſubſtance equal to other Kernels; and that the Pulp of the Berry, wherewith the Seed is ſurrounded, is of a more clammy ſticking nature than the Pulp of other Berries, for this very Purpoſe, that it might be of ſtrength ſufficient to fix the Seed on any Tree, how moveable or upright ſoever the Bough or Twig ſhould be whereon it chanced to light. And

And doubtleſs the Birds are (tho' not by their Dung) Sowers of this, as they are of many other Seeds, which they carry away for Food; but often drop in Places where they could otherwiſe never have come.

While I was writing this out for your Peruſal; I went to gather ſome *Miſſelto*-Berries (this being the Seaſon that they are ripe, and fitteſt to be ſown) and found a Leaf (which I ſend you) which hath a Seed ſticking thereon; doubtleſs by a caſual Fall out of the Bill of ſome Bird, that has broken the Berry as ſhe was eating it. There is both a dry ſtring of the Slime, and a dry ſpot of the ſame, upon the Leaf, that ſhow how the Seed was detained there, in this Caſe; and how it muſt be done in like manner any where elſe.

When I acquaint you, Sir, that I have ſown theſe Seeds, on near thirty ſorts of Trees and Shrubs, and yet never had above ten Plants, that held out the ſecond Year; you will not wonder, at the little ſucceſs, that others have had, in their trials of this Seed; and will alſo ſee the Reaſon, why I have not been able to make many other Experiments about the growth of this Plant. However, ſome Caſualties have furniſhed me with two or three; which I beg leave to relate; becauſe they do ſomewhat further explain the Nature of this Plant's growing.

One of my little Plants ſown in *Apr.* 1724. which was fixed at both Ends in its Arch-like Form, had in *Sept.* 1724, the middle part broken off; the two Ends keeping ſtill faſt to the Tree. Which ſhews, how firmly the two Ends adhere, while it is in that ſtate; and they both continued green ſome time, and then withered away.

2. That

2. That one Seed, which grew on a *Pear*-tree, in 171$\frac{8}{9}$, was the next Spring 17$\frac{19}{20}$, looſened from the Tree at one End, as the others were: Yet this ſeedling Sprout, never put out any Leaves at all; but continued in the ſame ſtate, neither bigger nor leſs, near ſix Years; that is, till it was broken off by chance in *July* 1725. This ſeems to me a very ſtrange thing: For, a ſeedling Plant (of any kind) is, but as it were an *Embryo*, till it have put forth Leaves.

3. My moſt thriving pair of Plants, of the Year 171$\frac{8}{9}$, being about three Inches in length, were on the 21*ſt* of *May* 1722, ſtruck off, by the falling of a Rake-handle againſt them. They took away with them. only the outmoſt thin skin of the Tree; and I could not ſee any ſigns, of deeper Rooting. But as I looked, now and then, on the Place, where the *Miſſelto* had grown, I thought, I obſerved the Bark to ſwell up a little; and on the 12*th* of *March* 172$\frac{1}{3}$, I perceived 3 or 4 little Budds, putting forth, and another Budd was put out by the 18*th* of *March*. They all grew on, to have Leaves that Summer; and now *Febr.* 172$\frac{6}{7}$, they are a Cluſter of Boughs, of 4 or 5 Joints in heighth, and bore Berries this Winter; whereas two others, on the ſame Tree, and which were alſo ſown at the ſame time, in 171$\frac{8}{9}$, and are 6 or 7 joynts in heighth, have not yet born any Berries

The thriving of theſe Plants, ſo well again, after they were broken off; made me reflect, on the *Druids* way, of cutting *Miſſelto* from the *Oak*, with a Golden Inſtrument; a Metal not apt to take a good Edge, and poſſibly, the bluntneſs of the Inſtrument, might be a means, to preſerve, a future growth, of the ſame Plant; which, doubtleſs, they as well as we, find to be very rarely upon the *Oak*. I might ſuggeſt ſome

Rea-

Reaſons for this Scarcity, from the Nature of that Bark; and I might obſerve many miſtakes, into which both Modern and Ancient Writers run, when they mention this Plant. But I have been ſo tedious already, that I will not trouble you, Good Sir, with any more, than this one Obſervation; which is, that there is almoſt every Year, on moſt *Miſſelto*-Buſhes, a viſible Proof, that the Kernel hath a vegetative Life in it: For when the Berries hang on till *May* or *June*, the Seed will make its little Shoot in the Berry, as the Kernels of Lemons, and you may ſee it coming out at the Eye of the Berry.

V. *An Account of a Pair of very extraordinary large Horns found in* Wapping *ſome Years ſince, with a probable Account, whence they came, and to what Animal they belonged. By Sir* Hans Sloane, *Baronet, Preſident of the Royal Society, and of the College of Phyſicians.*

MAny Years ſince, Mr. *Doyly*, (who was a great ſearcher after Curioſities, and gave Name to a ſort of Stuffs worn in Summer,) found a Pair of extraordinary large and ſtrangely ſhaped Horns in a Cellar, or Warehouſe, at *Wapping*, where they had ſuffered much by Worms and otherwiſe, being eaten pretty deep on their Surfaces, in many Places. They had lain there ſo long, that when he bought them, no body could inform him, either of the Country whence they came, or when, or how they had been lodged there. They reſembled in ſeveral things the Horns of Goats, which made many People think, that they had belonged to an Animal of that kind, in all likelihood as large as the *Mouſe-Dear* in *America* is of its kind. The Royal Society being informed of this matter, Mr. *Hunt*, their Operator at that time, made a deſign of them, on which Dr. *Hook* read a Lecture at a Meeting of the Society at *Greſham-College*. This Lecture and the Deſign, are, I think, loſt; but I remember, that he ſuſpected them to be the Horns of the *Sukotyro*, as the *Chineſe* call it,

it, or *Sucotario*, a very large and odd-ſhaped Beaſt, mentioned and figured by *Nieuhoff* in his Voyages and Travels to the *Eaſt-Indies* (a), where he gives the following Deſcription of it: *It is of the Bigneſs of a large Ox, with a Snout like a Hog, two long rough Ears, and a thick buſhy Tail. The Eyes are placed upright in the Head, quite different from other Beaſts; on the ſide of the Head next to the Eyes ſtand two long Horns, or rather Teeth, not quite ſo thick, as thoſe of the Elephant. It feeds upon Herbage, and is but ſeldom taken.* Many People thereupon went to ſee them at Mr. *Doyly*'s, who was offered a conſiderable Sum of Money for them, but refuſed it, and after a very ſevere Sickneſs, wherein I attended him, very much, as he thought, to his Advantage, he made me a Preſent of them.

They are both almoſt ſtreight for a conſiderable length, and then turning crooked, they run on tapering towards a ſmall and pretty ſharp end. They are not round, but compreſſed and flattiſh, and have large tranſverſe *Sulci*, or Furrows on their Surfaces, waved or undulated on their under parts. They differ ſome ſmall matter in largeneſs. Meaſuring one [Fig. 1.] from the great end, or *Baſis*, A B, where it was fixed to the Head, along the outward Circumference, I found the length ACD to be ſix Feet, ſix Inches and a half, the length by the Line B D was four Feet 5 ⅛ Inches, the Diameter of the *Baſis* A B was 6 ¼ Inches and its Circumference one Foot five Inches. This weighed 21 Pounds, 10 Ounces, and contained in the hollow part exactly five quarts of Water. In the other [Fig. 2.] the length of the outward Circumference A C D was ſix feet four inches, the Line B D four feet ſeven in-

(a) *P.* 360. *of the Engliſh Edition,*

 ches,

ches, the Diameter of the *Baſis* ſeven inches, and its Circumference one foot ſix inches. This weighed 21 pound, 13 ounces and a half, and contain'd in the hollow part 4 quarts and a pint, but would have held more, if it had not been very much broke at the large end.

The Commander of an *Eaſt-India* Merchant Ship upon ſeeing them, told me, that he had ſeen ſuch in the *Indies* on a large *Bufalo*'s Head. I am inclined to think, that they muſt belong to a very large ſort of Bulls or Cows, who are Natives of *Æthiopia*, and other of the midland Parts of *Africa*, and are mentioned by many of the Ancients, perhaps not without ſome fabulous Additions, though, which is ſtrange, very few of the Modern Writers take any notice of them.

Agatharchides *, a *Cnidian* who flouriſhed about the CL. *Olympiad*, near 200 Years before Chriſt, is the firſt

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* This *Agatharchides* lived in the time of *Ptolemeus Philometor*, and is mentioned by ſeveral of the Antients, as an Hiſtorian, and a Philoſopher of the Sect of the *Peripateticks*. Monſieur *le Clerc* (*Hiſtoire de la Medecine*, p. 387.) ranks him among the Phyſicians of that time, though that was not properly his Profeſſion; and the reaſon he gives for it is, becauſe he deſcribed, in one of his Books, a Diſtemper which had not been taken notice of by *Hippocrates*, nor any other Phyſician before him. We are indebted for this Particular to *Plutarch* (Sympoſiakῶn *Lib.* VIII. *Quæſt.* 9.) who ſays, upon the Authority of *Agatharchides*, *that the Nations about the Red Sea, amongſt many ſtrange Diſtempers, they are liable to, are troubled with a particular ſort of Serpents, (or Worms,) which breed in the Legs and Arms, where they ſometimes eat their way through, ſo as to ſhew their Head; but upon touching are apt to ſlide back, and creeping between the Interſtices of the Muſcles, occaſion intolerable Inflammations.* *Plutarch* adds, that this Diſtemper (which appears plainly to be the *Guinea-Worm*, or *Vena Medeni* of *Arabian* Writers, of which ſee my Natural Hiſtory of *Jamaica*, Vol. I. p. 126. and Vol. II. p. 190, 326. and which is a Diſtemper ſtill ſubſiſting, as well among thoſe very Nations, of which *Agatharchides* ſpeaks, as among the Negroes upon the Coaſts of *Guinea*, and the Inhabitants of ſeveral Parts of *Perſia*) had not been obſerved any where elſe, neither before, nor ſince the time of this Hiſtorian.

first among the Ancients, who mentions and deſcribes this large and voracious Bull; and it will appear by what follows, that moſt of the ſubſequent Writers have copied him. His Deſcription of this Animal, in ſome Remains of his Treatiſe of the Red Sea, which are extant in *Photius*'s *Bibliotheca*, (*b*) and were from thence printed in the *Geographiæ veteris Scriptores Græci minores*, publiſhed by Dr. *Hudſon*, is, according to the Tranſlation of *Laurentius Rhodomannus*, as follows, *De Tauro Carnivoro. Omnium, quæ adhuc commemoravi, immaniſſimum & maximè indomitum eſt Taurorum genus, quod carnes vorat, magnitudine craſſius domeſticis, & pernicitate antecellens, inſigniter rufum. Os ei ad aures uſque deductum. Viſus glauco colore magis rutilat quam Leoni. Cornua alias non ſecus atque aures movet, ſed in pugna, ut firmo tenore conſiſtant, facit. Ordo pilorum inverſus, contra quam aliis animantibus. Beſtias etiam validiſſimas aggreditur, & cæteras omnes venatur, maximéque greges incolarum infeſtos reddit malefficio. Solum eſt lancea & arcu invulnerabile, quod in cauſa eſt, ut nemo id ſubigere, (quamvis multi id tentarint,) valuerit. Ideò rectè putatur, etiam à Troglodytis, fortitudine leonis, & velocitate equi, & robore Tauri præditum, ferróque cedere neſcium.* *Diodorus Siculus* (*Biblioth. Lib.* III.) hath barely, and almoſt Word for Word, tranſcribed *Agatharchides*, and hath added only the following Particulars; that, the Eyes of this Animal are ſhining at Night; that after it hath killed other Beaſts, it devours them, and that, in its Attacks upon Flocks of Cattle, it is not to be terrified, either by the Strength of the Shepherds, or the great Number of Dogs. The following ſhort Paſſage

(*b*) P. 1364. Cap. XXXIX.

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relating to this ſame Animal is taken out of *Strabo* (c). *Sunt & ibidem* (in Arabia) *tauri feri, ac qui carnem edant, noſtros & magnitudine & celeritate longè exſuperantes, colore ruffo.* *Pliny, Hiſtor. Nat.* Lib. VIII. C. XXI. ſeems likewiſe to have copied *Agatharchides*: His Words are, *Sed atrociſſimos habet* (Æthiopia) *Tauros ſylveſtres, majores agreſtibus, velocitate ante omnes, colore fulvos, oculis cæruleis, pilo in contrarium verſo, rictu ad aures dehiſcente, juxta cornua mobilia, tergori duritia ſilicis, omne reſpuens vulnus. Feras omnes venantur: Ipſi non aliter, quam foveis capti, feritate ſemper intereunt.* In the XLV Chapter of the ſaid 8th Book of *Pliny*'s Natural Hiſtory, he mentions a ſort of *Indian* Oxen, *Boves Indici, quibus Camelorum altitudo traditur, cornua in latitudinem quaternorum pedum.* It is not unlikely, but that theſe *Indian* Oxen are the ſame with the *Æthiopian* ones above deſcribed; eſpecially if we ſuppoſe, that the Tranſcribers of *Pliny* have by miſtake wrote *latitudinem* inſtead of *altitudinem*. *Solinus* (d) hath barely copied *Pliny*, with this difference only, that he calls them *Indicos Tauros*, whereas *Pliny* himſelf hath deſcribed them amongſt the *Æthiopian* Beaſts, which might very well happen, *Æthiopia* being reckoned, by ſome of the Antients, as part of *India*. The deſcription of *Ælianus* (e) agrees perfectly with that of *Agatharchides*, of whom, it ſeems, he alſo borrowed it; only he fixes the Size of theſe extraordinary Oxen to twice the Bigneſs of the common *Grecian* Ox. There is another Paſſage in *Ælianus* (f), which ſeems to relate, both to this large

(c) *Geogr. Lib. XVI. p.* 775. *Edit. Caſaubon.*
(d) Polyhiſtoris Cap. I. II. p. 58. Edit. Salm.
(e) Hiſt. Animal Lib. XVII. c. 45.
(f) Hiſt. Animal Lib. III. c. 34

kind

kind of *Æthiopian* Oxen, and the Horns, now in my poſſeſſion. His Words are, *Ptolomæo ſecundo ex India cornu allatum ferunt, quod tres amphoras caperet : Unde conjicere poſſumus bovem illum, à quo ejuſmodi tantum cornu extitiſſet, maximum fuiſſe.* *Lúdolfus*, in his *Hiſtoria Æthiopica*, (g) ſpeaking of the large *Ethiopian* Oxen, conjectures, that they are the *Taurelephantes*, which *Philoſtorgius*, a *Cappadocian*, ſays, (*h*) were brought to *Conſtantinople* in his time, where he ſaw them. The Words of *Philoſtorgius*, as tranſcribed by *Ludolf* in his *Commentarius ad Hiſtoriam ſuam Æthiopicam* (*i*), are, *Habet & Terrailla maximos & vaſtiſſimos Elephantas ; imo & Taurelephantes, ut vocantur, quorum genus quoad cætera omnia bos maximus eſt, corio verò coloreque elephas, & fermè etiam magnitudine.*

From all theſe ſeveral Writers it appears, that there is in *Æthiopia* (and probably the midland parts of *Africa*, where Travellers ſeldom come) a very large Animal of the *bovinum genus*, at leaſt twice as big as our Bulls or Oxen, with Horns proportionably large, but otherwiſe differing from them in many reſpects. It is true, that in the accounts of uncommon things, given by antient Writers, an allowance muſt ſometimes be made for ſomething fabulous mix'd with what is true, which is the leſs ſurprizing, as they had many things only by hear-ſay: but as to this large ſort of Bulls, it is confirmed by modern Writers, that there is ſuch an Animal in thoſe Countries, though there is none, I know of, that hath given a deſcription of it in the leaſt ſatisfactory. *Ludolf* in

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(*g*) Lib. I. c. 10.
(*h*) Lib. III. c. 11.
(*i*) P. 145.

his

his *Hiſtoria Æthiopica*, (*k*) ſays, that there are in *Æthiopia* Bulls of an uncommon ſize, twice as large as thoſe in *Hungary* and *Ruſſia*; and that having ſhewn ſome of the largeſt Oxen in *Germany* to *Gregory*, an *Abiſſynian* (from whoſe Writings and informations he collected the Materials for that Work) he ſaid, they were but of a midling ſize. The Letters of the Jeſuits frequently mention the largeneſs of theſe Oxen, and the ſaid *Ludolf* cites the following paſſage out of a Letter of *Alphonſus Mendezius*, Patriarch of *Æthiopia*, dated *June* 1, 1626. *Buoi grandiſſimi, di corna ſmiſuratamente groſſe é lunghe, talmente, che nella corna di ciaſcuno di eſſe potea capire un otre piccolo di vino:* that is, *very large Oxen, with vaſtly thick and long Horns, one of which would contain a large Uter of Wine.* *F. Bernier*, in his account of the *Great Mogul*'s Country, (*m*) ſays, that among many Preſents, which two *Æthiopian* Ambaſſadors ſhould have preſented to *Aurengzeb*, there was a prodigious large Horn of a Bull, full of Civet, which having been meaſured by him, he found the *Baſis*, or large end, to be half a foot in Diameter. This Horn, as *Bernier* farther obſerves, was brought by the Ambaſſadors to *Dehli*, the place of Reſidence of the *Great Mogul*; but it was not preſented to him, becauſe, being diſtreſs'd for Money, they had ſold the *Civet* out of it, long before they came thither.

Upon the whole, it ſeems to me, that theſe Horns, and likewiſe that mentioned by *Bernier*, are the Horns of a large ſort of Bulls or Cows, in *Æthiopia*, and the

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(*k*) Lib. I. c. 10.
(*l*) Comment. in Hiſt. Æthiop. p. 145.
(*m*) Tome. II. p. 43.

inward

inward parts of *Africa*, which, in all likelihood, is the ſame with that deſcribed by *Agatharchides*, *Pliny*, and thoſe other ancient Writers mention'd above. But I cannot as yet, for want of a more accurate deſcription, be certain, whither it is the ſame with the *Sukotorio*, or *Sukotyro* of *Nieuhof* (*n*), though there is a good deal of reaſon to think, that it is. *Geſſner* (*o*) ſpeaks of a very large Horn, which was hung by a Chain to a Pillar in the *Minſter*, or Cathedral, of *Strasburgh*, and which is not unlikely to be of the ſame ſort with theſe. He ſays, that being meaſured along the outward circumference, it was found to be four *Roman* Yards long, and he conjectures it to have been the Horn of a large old *Urus*, which was hung up there, for its monſtrous ſize, perhaps two or three hundred Years before his time. As to theſe, which are now produced before the *Royal Society*, 'tis very likely, that when the *Engliſh* had a great Commerce at *Ormus*, they were brought thither from ſome neighbouring Country, and afterwards carried over into *England* by ſome curious Perſon.

(*n*) *Loc. ſupra citato.*

(*o*) *Icones Animal. Quadrup. Edit. 2d. Tigur. 1560. p. 34.*

VI. *A Demonſtration of the* 11*th Propoſition of Sir* Iſaac Newton's *Treatiſe of* Quadratures. *By Mr.* Benjamin Robins.

THIS Propoſition conſiſts of two Parts: The firſt is as follows.

Let there be any Curve A D I, whoſe Abſciſſe A B ſhall be denoted by z, and its Ordinate B D by y; which may be related in any manner to the Abſciſſe. And calling this the firſt Curve, let other Curves AEK, AFL, AGM, AHN, *&c.* be formed to the common Abſciſſe A B, or z, by making the Ordinate B E of the ſecond Curve always equal to the Area A B D of the firſt divided by Unity; the Ordinate BF of the third equal to the Area A B E of the ſecond divided by Unity; the Ordinate B G of the fourth equal to the Area A B F of the third divided by Unity; and ſo on continually. Suppoſe now, that other Curves AOS, APT, AQV, ARW, be deſcribed to the ſame common Abſciſſe A B or z; in which Curves the Ordinate B O of the Curve A O S ſhall be equal to $z\,y$, the Ordinate B P of the Curve A P T equal $z^2\,y$, the Ordinate B Q of the Curve A Q V equal to $z^3\,y$, the Ordinate B R of the Curve A R W equal to $z^4\,y$, *&c.* And let the whole Area A C I be denoted by A, the Area A C S by B, the Area A C T by C, the Area A C V by D, the Area A C W by E, *&c.* Then the Series of Curves A D I, A E K, A F L, A G M, A H N are thus meaſured:

The

The Area of the firſt Curve ADI is $=A$
——of the ſecond AEK is $= zA - B$
——of the third AFL $= \frac{zzA - 2zB + C}{2}$
——of the fourth AGM $= \frac{z^3A - 3z^2B + 3zC + D}{6}$
——of the fifth AHN $= \frac{z^4A - 4z^3B + 6z^2C - 4zD + E}{24}$

and ſo on perpetually. Here in all the Curves following the firſt, the Index of the higheſt Power of z is always the Number which expreſſes the Diſtance of the Curve from the firſt, and afterwards decreaſes regularly by Unity; the firſt Term is multiplied into A, the ſecond into B, the third into C, the fourth into D, and ſo on; the Coefficients are the ſame as in a Binomial raiſed to the higheſt Power of z, and the Diviſor is ſo many Terms of this Progreſſion $1 \times 2 \times 3 \times 4 \times 5 \times 6$ &c. as is expreſs'd by a Number equal to the higheſt Index of z. Otherwiſe ſuppoſing x to repreſent the Diſtance of the Curve to be meaſured from the firſt; then the Area ſought will be found by extending $\overline{z-1}|^n$ into a Series, and multiplying the firſt Term by A, the ſecond by B, the third by C, the fourth by D, &c. and dividing the whole by $n \times \overline{n-1} \times \overline{n-2}$ &c. continued to Unity.

SECOND PART.

Suppoſing the firſt, ſecond, third, &c. Curves to be the ſame as before: Let t denote the whole Abſciſſe AC, and put x for BC. Then deſcribe the Curves CXA, CYA, CZA, CΓA, where BX ſhall be equal to xy, BY $= x^2 y$, BZ $= x^3 y$, BΓ $= x^4 y$, &c. This being done, and in the Series of Curves CIDA,

CXA, CYA, CZA, CTA, &c. the firſt Area CIDA being put equal to P, the ſecond CXA equal to Q, the third CYA=R, the fourth CZA=S, the fifth CTA=T, &c. the whole Areas of the aforeſaid Series of Curves are alſo determin'd as follows.

The firſt AIC=P
The ſecond AKC=Q
The third ALC=$\frac{1}{2}$ R
The fourth AMC=$\frac{1}{6}$ S
The fifth ANC=$\frac{1}{12}$ T.

Here the Area's P,Q,R,S,T are divided by Numbers produced by multiplying as many Terms of this Series 1×2×3×4×5 &c. together, as in the former Caſe.

Demonſtration of the Firſt Part.

Let the Area ABD be denoted by *a*, the Area ABO by *b*, ABP by *c*, ABQ by *d*, and ABK by *e*. Then it is evident, that

The Fluxion of the Area ABD is= $\dot{x}\times BD=\dot{x}y=\dot{a}$
The Fluxion of the Area ABO is= $\dot{x}\times BO=\dot{x}xy=\dot{b}$
The Fluxion of the Area ABP is= $\dot{x}\times BP=\dot{x}x^2y=\dot{c}$
&c. &c.

Hence

$\dot{x}\times\dot{a}$ is $(=\dot{x}xy)=\dot{b}$
$x^2\times\dot{a}$ is $(=\dot{x}x^2y)=x\dot{b}=\dot{c}$
$x^3\times\dot{a}$ is $(=\dot{x}x^3y)=x^2\dot{b}=x\dot{c}=\dot{d}$.

Or generally,

$x^n\times\dot{a}=x^{n-1}\times\dot{b}=x^{n-2}\times\dot{c}=x^{n-3}\times\dot{d}$, &c.

Now

Now as $\dot{z} \times$ ABD or $\dot{z} \times a$ is $=$ Fluxion of ABE, if you add to the firſt Part $z \times \dot{a}$ $(= \dot{z}z)$ and its Equal $\dot{b}$ to the other Part, it follows, that

$$\left.\begin{matrix} \dot{z} \times a \\ + z \times \dot{a} \end{matrix}\right\} = \text{Fluxion of ABE} + \dot{b}$$

And taking the Fluents
$z \times a = \text{ABE} + b$ or $\text{ABE} = z \times a - b$; and when z or AB becomes $=$ AC, then ABE becomes ACK, and a and b become A and B; therefore ACK is $= z \times \text{A} - \text{B}$.

Again, The Ordinate BF of the next Curve is equal to ABE, which has been proved equal to $z \times a - b$. Conſequently the Fluxion of ABF is $= \dot{z}z\,a - \dot{z}b$; and if you add to the firſt Part of this Equation $\frac{1}{2} z^2 \times \dot{a} - \dot{z}b$ $(= \frac{1}{2} z^2 \dot{z} y - \dot{z} z^2 y = -\frac{1}{2} \dot{z} z^2 y)$ and its Equal $-\frac{1}{2}\dot{c}$ on the other, it follows, that

$$\left.\begin{matrix} \dot{z}z\,a - \dot{z}b \\ + \frac{1}{2} z z \dot{a} - z\dot{b} \end{matrix}\right\} = \text{Fluxion of ABF} - \tfrac{1}{2}\dot{c}$$

And taking the Fluents
$\frac{1}{2} z^2 a - z b = \text{ABF} - \frac{1}{2} c$; or by tranſpoſing
$\text{ABF} = \frac{z^2 a - 2 z b + c}{2}$; or ſuppoſing z equal to AC,
$\text{ACL} = \frac{z^2 \text{A} - 2 z \text{B} + \text{C}}{2}$.

The Ordinate BG is equal to ABF, which has been proved equal to $\frac{z^2 a - 2 z b + c}{2}$: Therefore the Fluxion of ABG is equal to $\frac{z \dot{z}^2 a - 2 \dot{z} z b + \dot{z} c}{2}$

And

And adding $\frac{1}{6}\dot{z}z^3a - \frac{1}{2}\dot{z}z^2b + \frac{1}{2}\dot{z}c$ $(= \frac{1}{6}\dot{z}z^3y - \frac{1}{2}\dot{z}z^3y + \frac{1}{2}\dot{z}z^3y = \frac{1}{6}\dot{z}z^3y)$ on one side, and its Equal $\frac{1}{6}\dot{d}$ on the other, it will be

$$\left.\begin{array}{l}\frac{1}{2}\dot{z}z^2a - \dot{z}zb + \frac{1}{2}\dot{z}c \\ + \frac{1}{6}z^3\dot{a} - \frac{1}{2}z^2\dot{b} + \frac{1}{2}z\dot{c}\end{array}\right\} = \text{Fluxion of } ABG + \frac{1}{6}\dot{d}$$

And taking the Fluents
$\frac{1}{6}z^3a - \frac{1}{2}z^2b + \frac{1}{2}zc = ABG + \frac{1}{6}d$; and tranſpoſing,
$ABG = \frac{z^3a - 3z^2b + 3zc - d}{6}$; or ſuppoſing z equal to AC; then $ACM = \frac{z^3A - 3z^2B + 3zC - D}{6}$.

In the ſame manner the Fluxion of ABH is equal to $\frac{\dot{z}z^3a - 3\dot{z}z^2b + 3\dot{z}zc - \dot{z}d}{6}$; and adding on one ſide $\frac{1}{24}z^4\dot{a} - \frac{1}{6}z^3\dot{b} + \frac{1}{4}z^2\dot{c} - \frac{1}{6}z\dot{d}$, and its Equal $-\frac{1}{24}\dot{e}$ on the other, it becomes

$$\left.\begin{array}{l}\frac{1}{6}\dot{z}z^3a - \frac{3}{6}\dot{z}z^2b + \frac{3}{6}\dot{z}zc - \frac{1}{6}\dot{z}d \\ + \frac{1}{24}z^4\dot{a} - \frac{1}{6}z^3\dot{b} + \frac{1}{4}z^2\dot{c} - \frac{1}{6}z\dot{d}\end{array}\right\} = \text{Fluxion of } ABH - \frac{1}{24}\dot{e}$$

And taking the Fluents
$\frac{1}{24}z^4a - \frac{1}{6}z^3b + \frac{1}{4}z^2c - \frac{1}{6}zb = ABH - \frac{1}{24}e$: therefore $ABH = \frac{z^4a - 4z^3b + 6z^2c - 4zd + e}{24}$;
Or $ACN = z^4A - 4z^3B + 6z^2C - 4zD + E$, ſuppoſing z equal to AC. In like manner you may proceed to meaſure any of theſe Curves: and you will always find their Value the ſame as is expreſſed in the Propoſition.

De-

Demonſtration of the Second Part.

Suppoſe any Curve whoſe Diſtance from the firſt is denoted by n; then the Curve whoſe Abſciſſe is BC or x, and its Ordinate $x^n y$ divided by $n \times \overline{n-1} \times \overline{n-2} \times \overline{n-3}$ &c. continu'd to Unity will be equal to it, when x is equal to AC or t.

It is evident, that when the Areas ABD, ABO, ABP, ABQ, ABR, &c. decreaſe, the Areas BCID, BCSO, BCTP, BCVQ, BCWR increaſe reſpectively; and conſequently the Decrements of the Areas ABD, ABO, ABP, &c. or their Fluxions with a negative Sign, are the Increments or Fluxions of the Areas BCID, BCSO, BCTP &c. that is, calling the Area BCID, α; the Area BCSO, β; the Area BCTP, γ; BCVQ, δ; BCWR, ε: then $\dot{\alpha} = -\dot{a}$, $\dot{\beta} = -\dot{b}$, $\dot{\gamma} = -\dot{c}$, $\dot{\delta} = -\dot{d}$, $\dot{\varepsilon} = -\dot{e}$.

Now the Fluxion of the Curve, whoſe Abſciſſe is $= x$, or BC, and its Ordinate $= x^n y$ is $\dot{x}\, x^n y$; that is, equal to $\dot{x}\, y \times \overline{t-z}|^n$; x being $= t - z$; or ſince the Increment of x, or $\dot{x}$ is equal to the Decrement of z, or $-\dot{z}$, the Fluxion of the ſame Curve is equal to $-\dot{z}\, y \times \overline{t-z}|^n = -\dot{z}\, y$ in $t^n + n \times t^{n-1} z + n \times \frac{n-1}{2}\, t^{n-2} \times z^2$ &c. $= -t^n \dot{z}\, y + n\, t^{n-1} z \dot{z} y - n \times \frac{n-1}{2}\, t^{n-2} \dot{z} z^2 y$, &c.

that is, $= t^n \times -\dot{a} - n\, t^{n-1} \times -\dot{b} + n \times \frac{n-1}{2}\, t^{n-2} \times -\dot{c}$, &c.

or $= t^n \dot{\alpha} - n\, t^{n-1} \beta + n \times \frac{n-1}{2}\, t^{n-2} \dot{\gamma}$, &c. and taking the Fluents, the Area of the Curve, whoſe Abſciſſe is x, or BC, and Ordinate $x^n y$, is equal to $t^n \alpha - n\, t^{n-1} \beta + n \times \frac{n-1}{2}\, t^{n-2} \gamma$ &c. But when x is equal

to

to AC, then z, β, γ, &c. will be equal to A, B, C, &c. as is very evident; consequently the Area of the Curve whose Abscisse is x, and Ordinate $x^n y$, when x is = AC, is $t^n A - nt^{n-1} B + n \times \frac{n-1}{2} \times t^{n-2} C$ &c. that is equal to $\overline{t-1}|^n$ thrown into a Series, and the first Term multiplied by A, the second by B, the third by C, &c. But $\overline{t-1}|^n$ thrown into a Series, and the first Term multiplied by A, the second by B, the third by C, &c. and then the whole divided by $n \times \overline{n-1} \times \overline{n-2}$, &c. continued to Unity, is equal to the Area of the Curve, whose Place in the Series is denoted by n: Therefore the Area of the Curve, whose Abscisse is equal to x, and its Ordinate to $x^n y$, taken when x is equal to AC, and divided by $n \times \overline{n-1} \times \overline{n-2} \times \overline{n-3}$ &c. continued to Unity, is equal to the Area of a Curve whose Place in the Series is denoted by n; that is, Q, which is the Area of a Curve, whose Abscisse is x, and Ordinate $x y$ taken when x is = AC, is equal to the second Curve AKC; half R, which is the Area to the Abscisse x, and Ordinate $x^2 y$, taken in the same manner, is equal to the third Curve ALC; $\frac{1}{3}$ S, which is a like Area to x and $x^3 y$, is equal to the fourth Curve AMC; $\frac{1}{24}$ T, the Area to x and $x^4 y$, x being equal to AC, is equal to the fifth Curve ANC; and so on perpetually Q.E.D.

VII. *Muris*

VII. *Muris Alpini Anatome.* *A* Joh. Jacobo Scheuczero, Tigurino, *M. D. R. S. S.*

RElictis abdominalibus muſculis, aperto abdomine, in conſpectum venit illico omentum pingue admodum, cujus tractus pinguedinoſi vaſis ſanguiferis epiploicis intertexti pulcherrimum formant rete; ſed eſt hæc omenti pinguedo concreta magis & compactior ea, quæ mox deſcribenda venit, ac veluti emaciata, ut omninò judicare liceat, reſorptas eſſe per hyemem durante ſomno fluidiores oleoſi hujus liquidi partes per venam portæ, ut inſervirent tum ſecretioni bilis, quâ veſica bilaria fuit admodum turgida, tum nutrimento ipſius corporis.

Ex utroque hypogaſtrii latere copioſiſſima conſpicitur & laxioris, quàm illa omenti, conſiſtentiæ pinguedo, quæ inde à renibus ſeſe protendit ad inguen, formans veluti alterum, imò duplex omentum, quæ pinguedo, uti & illa meſenterii, quæ ductum inteſtinorum omnium comitatur, adeſt tum ad lubricanda viſcera abdominalia, tum in ſucci nutritii amplum penuarium.

Ad duodeni latus pro more, imò ultra ejus curvaturam, ſeſe protendit pancreas, idque valde extenuatum, in quo macilentiæ ſtatu vidimus omnes alias corporis glandulas, ſpeciatim eas, quæ muſculis interjacent; in his animantibus tardiſſima eſt ſanguinis circulatio, tardiſſimæ omnes corporis ſecretiones, nulla interim ſeri vel lymphæ, quæ tardam quidem, ſed tamen ſucceſſivam habet ſecretionem, revectio, ut tandem ſanguis omni penè ſero orbetur; hinc non mirum, quod

deficiente lympha ſecernenda, glandula pancreatis, & ſic quoque aliæ debeant emaciari.

Aperto duodeno in conſpectum prodit bilis ſpumoſa, quæ ipſa indicat ſeri defectum.

In fœmella hac uteri cornua ſeſe protendunt ad duos pollices Pariſinos. tubæ pertenues filo vix craſſiores ad $\frac{1}{3}$ pollicis, ovaria 2 lineas circiter longa, unam lata, alba, ſub microſcopio pellucida, cum prominentibus ovulis diſtinctæ diaphaneitatis.

Hepatis ſatis magni lobi ſunt ſex, & aliqui horum in duos veluti ſubdiviſi per inciſuram: Lobo infimo mediante membrana connectitur cum rene dextro.

Renes pinguedine toti ſunt obſiti.

Renes ſuccenturiati ad latus venæ cavæ ſupra emulgentes corpuſcula ſunt flavicantia tres circiter lineas longa vix dimidiam lata, hepar inter & renes conſpicua; ſed uti aliæ glandulæ, macilentæ.

Ventriculi ſtructura eſt, uti in animalibus aliis prohibitis & carnivoris, ſimplex membranoſa, licet hoc animal ſit herbivorum, figura ejus extat litera *A*.

Singularis ſtructuræ eſt fiſtula inteſtinalis in confinio inteſtinorum tenuium & craſſorum.

Ingreſſus ilei in colon in diametro habet duntaxat tres lineas; colon è regione ilei 2 lineas. Sed quod incurvatur ibidem cœcum, eſt ex ampliſſimis, 2 pollicum in diametro. Memorabilis eſt valvula coli dicta, annuli ad inſtar rotunda, ſingularis prorſus ſtructuræ. Exilis quippe admodum eſt.

Ingreſſus ilei veluti inter duas tunicas conniventes, ita ut nullus prorſus concedi poſſit excrementis ad inteſtina tenuia regreſſus. Et membranæ duæ, quæ mutuo occurſu valvulam illam formant, rhomboidalis ſunt figuræ. Adſunt præterea aliæ valvulæ conniventes annulares, & veluti in ramos protenſæ tres quatuorve principales verſus inteſtinum cœcum. Illuſtrat hæc ob-

obſervatio egregie uſum inteſtini cœci, quod in infantibus recens natis ordinariè eſt capacius. Inſervit nempe id diverticuli loco excrementis per novem menſium decurſum in inteſtinis colligi, nec per ea excerni ſolitis. Par hîc eſt ratio animantium quæ per hyemem integram in montium cavernis dormiunt. Nulla per illud tempus fit excrementorum egeſtio, & tamen non obſtante hâc tardiſſimâ circulatione atque ſecretione, nullâve ciborum aſſumptione, collectio fit eorundem, quæ ne inteſtina utriuſque generis infarciant nimium, amandantur ad cœcum, ibique ad uſque vernum tempus manent; regreſſus autem ex eodem ad colon impeditur imprimis per valvulas ante deſcriptas. Structuram hanc inteſtinorum memoratorum integram oculis exhibet figura B ; valvulæ autem coli & aliarum conniventium fig. C. Ex myographia ea imprimis contemplanda duximus, quæ actiones hujus animalis diſtinguunt ab aliis aliorum. Immediatè ſub cute craſſus eſt & fortis muſculus platyſma myoides dictus, qui colli partem anteriorem & lateralem, imò & totum muſculum maſſeterem faſciæ inſtar involvit. Ad ipſam uſque articulationem humeri cum cubito ſe protendens labio tum inferiori, tum ſuperiori inſeritur, & motum frequentem pedum anteriorum flexorium ſimul & labiorum inſigniter adjuvat. Hæc quippe Animalia cibos pedum anteriorum beneficio, veluti manibus apprehendentia, ori admovent, & terram illorum ope ſuffodiunt.

Glandulæ thyroideæ ad laryngis latera magnæ, haud dubiè ad irrigationem muſculorum vicinorum, quorum uſus admodum eſt frequens, quem ipſum in finem paſſim aliis muſculis interjacent alia corpora glanduloſa paſſim memoranda.

Sternohyoideus & ſternothyroideus in ordinario ſunt ſitu.

Fortiſſimus & tendinoſus valde eſt maſſeter, maximi in his animalibus uſus.

Fortiſſimus item depreſſor maxillæ inferioris digaſtricus, qui baſi oſſis hyoidis & ejus cornibus forti item tendine à proceſſu ſtyloide ortus in maxillam inferiorem ad mentum ferè uſque inſeritur, cujus venter anterior imprimis eſt craſſus & poſteriori quadruplo fortior.

Loco geniohyoidei externi adeſt ſtatim ſub mento maſſa carnea muſculoſa craſſa, triangularis inde à maxillæ inferioris concurſu, ſive angulo, ſeſe in quinque lineas lateraliter protendens, quæ maxillam inferiorem imprimis connectere videtur. Poſt hunc baſi linguæ inſternitur muſculus latus tranſverſis fibris carnoſis à maxillæ inferioris parietibus internis ortus, qui tum in mediam linguæ baſin, tum in os hyoides inſeritur; qui etiam ſupplere videtur geniohyoideum externum. Tenuis ille eſt & à ſubſtrato geniohyoideo interno, ſive geniogloſſo vix ſeparandus.

Styloglosſus fortis quoque eſt muſculus, carnoſoque principio à proceſſu ſtyloide ortus in fortem deſinens tendinem.

Conſpicui valde ſunt muſculi cricothyroidei item hyrohyoidei.

Ad latera gulæ & aſperæ arteriæ glandulæ ſunt utrinque ad digiti ferè longitudinem protenſæ, quæ nonniſi elevata aſpera arteria & gula in conſpectum veniunt, & lubricandis his partibus inſervire videntur.

Fortis eſt muſculus rectus anticus major ejuſdem planè ſtructuræ uti in homine.

Ceratohyoides triangularis muſculus à cornibus oſſis hyoidis ortus in baſin ejus inſeritur.

Thy-

Thyroarytænoideus ſub ſphinctere gulæ conſpicuus ad id ſpeciatim inſervit, ut clangorem his animalibus proprium efficiat conſtringendo fortiter Arytænoideas cartilagines.

Muſculi pterygoidei ſunt admodum fortes.

Ad ſupra jam deſcriptum platyſma myodes adnotandum porro, portionem craſſam ſatis inſeri ſpinæ ſcapulæ, ſed quoque eidem muſculo tranſverſim implantari alium tenuem trapezio inſtratum, qui proin integer ex variis partibus coadunatus muſculus inſervit motui labiorum, flectendo cubito, levandæ ſcapulæ & antrorſum ducendis brachiis, quorum opera hæc animalia terram ſuffodiunt, aliaſque res ſuas peragunt.

Trapezius eodem prorſus modo ſe habet ut in homine.

Rhomboidis loco adeſt muſculus fortis ſub trapezio in conſpectum veniens, qui oritur non ſolum à ſuperioribus ſpinis vertebrarum dorſi, & inferioribus colli, ſed omnibus omninò ſpinoſis tranſceſſibus vertebrarum colli, & inſuper ab occipite, ita ut ſcapulam non ſolum retrorſum moveat, ſed etiam elevet, ſicque patientiæ muſculum in actione ſua adjuvet.

Inſertio hæc rhomboidis in occiput obſervatur quoque in quadrupedibus aliis, & haud dubiè eum in finem adeſt, ut caput pronum pendens tanto melius ſuſtineatur.

Patientiæ muſculus tenui principio oritur ab apophyſi tranſverſa primæ vertebræ, & in principium ſpinæ ſcapulæ inſeritur.

Hujus actionem adjuvat alius eodem principio ortus, qui ad finem ſpinæ dictæ inſeritur, qui duo proin junctim ſumpti totam ſcapulam attollunt.

Ser-

Serratum inter anticum majorem, qui eodem modo ſe habet uti in homine, & ſubſcapularem adeſt glanduloſa caro magna & lata fortibus his muſculis lubricandis inſerviens.

Serratus anticus major non ſolum dentatas habet origines à coſtis, ſed etiam fortes & craſſas propagines arceſſit à proceſſibus tranſverſis vertebrarum colli.

Aniſcalptor adeſt duplex unus alteri inſtratus, inferior & latior atque fortior dentatis veluti proceſſibus ab ipſis coſtis, imprimis inferioribus oritur, & in eriori ſcapulæ coſtæ in tranſitu firmiter annexus lata ſatis baſi humero uti in homine inſeritur, & præterea propaginem mittit tenuem ad ipſum olecranon, ita ut non ſolùm humerum retro deorſum trahat, ſed etiam cubiti extenſioni inſerviat.

Muſculi humeri infraſpinatus, ſubſcapularis, & rotundus uterque eodem modo ſe habent uti in homine.

Deltoides duplex eſt. Ea pars, quæ oritur à clavicula & acromio in flexuram cubiti inſeritur, adeoque non tam ad humeri, quàm ad cubiti muſculos pertinet; altera verò portio, quæ ab acromio & ſpina ſcapulæ oritur, exteriorem humeri ſitum obtinet in medium circiter humerum inſerta; hunc proin non ſolùm ſurſum movet, ſed & extrorſum abducit.

Coracobrachiæus tenuis eſt muſculus ejuſdem ſitus & uſus ut in homine.

Inter extenſores cubiti primus eſt tenuis, reliquiis faſciæ ad inſtar inſtratus, à coſta ſcapulæ ortus; ſequuntur duo alii fortes, quorum unus etiam à coſta ſcapulæ oritur, alter à ſuperiore parte humeri, qui aliquandiu progreſſi ſeſe uniunt & forti tendine in olecranon inſeruntur.

Sub his alius latet itidem fortis, qui à ſumma humeri parte ortus cum anconæo ſeſe unit.

Hic

Fig. 5.

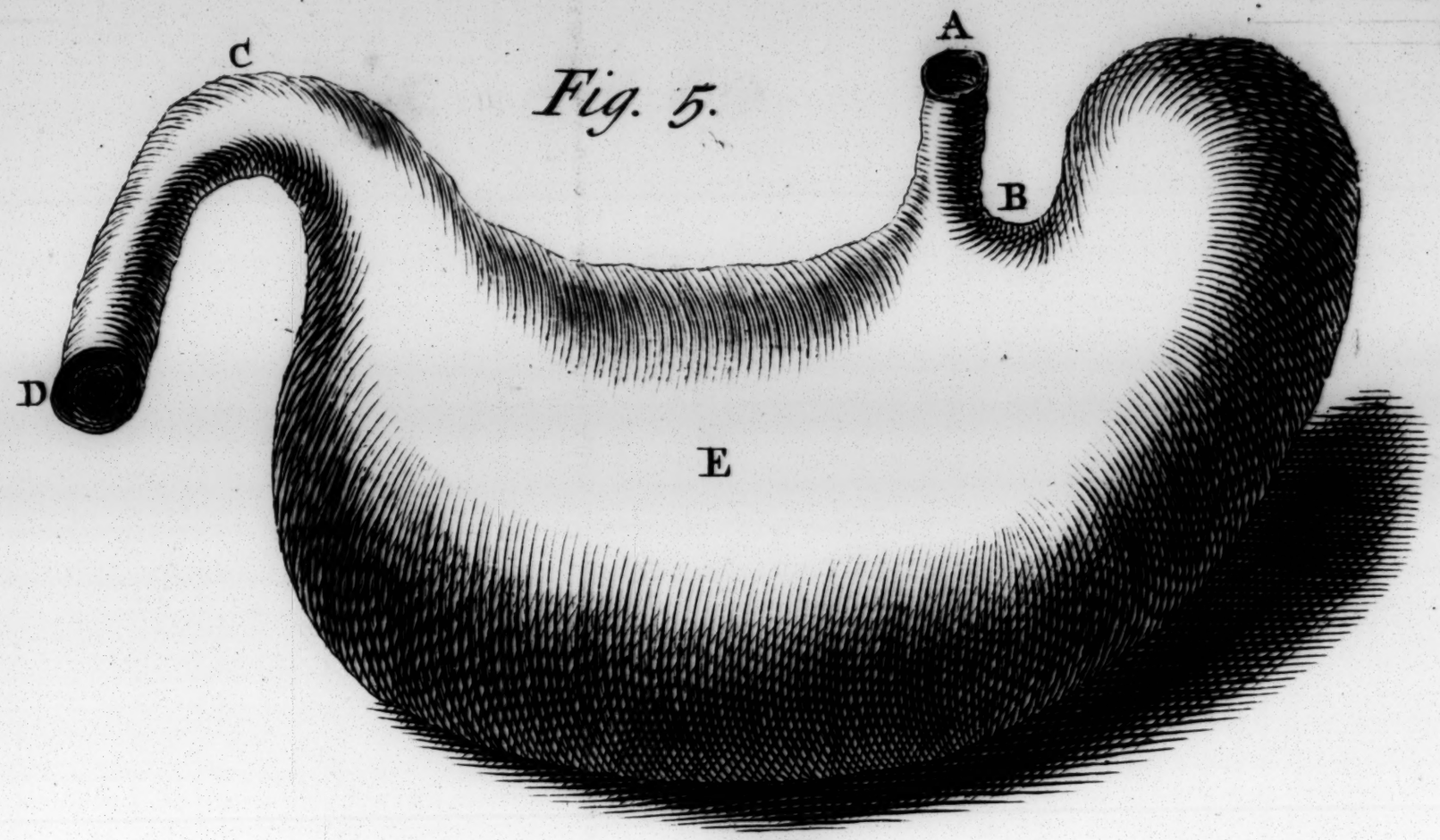


Fig. 7.

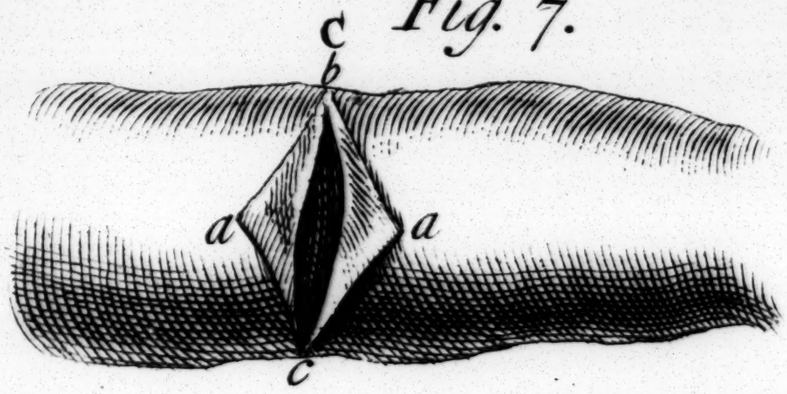


Fig. 6.

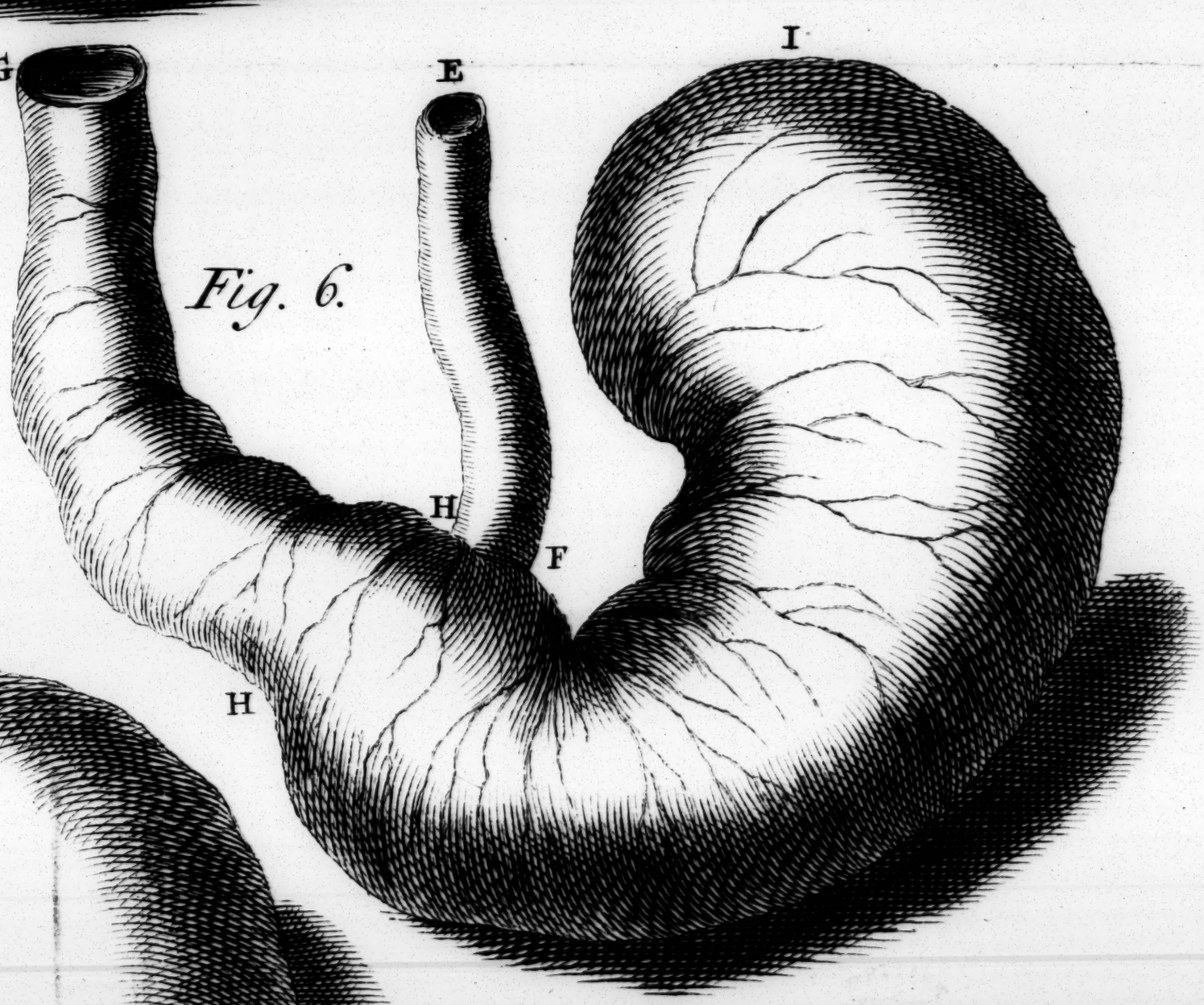
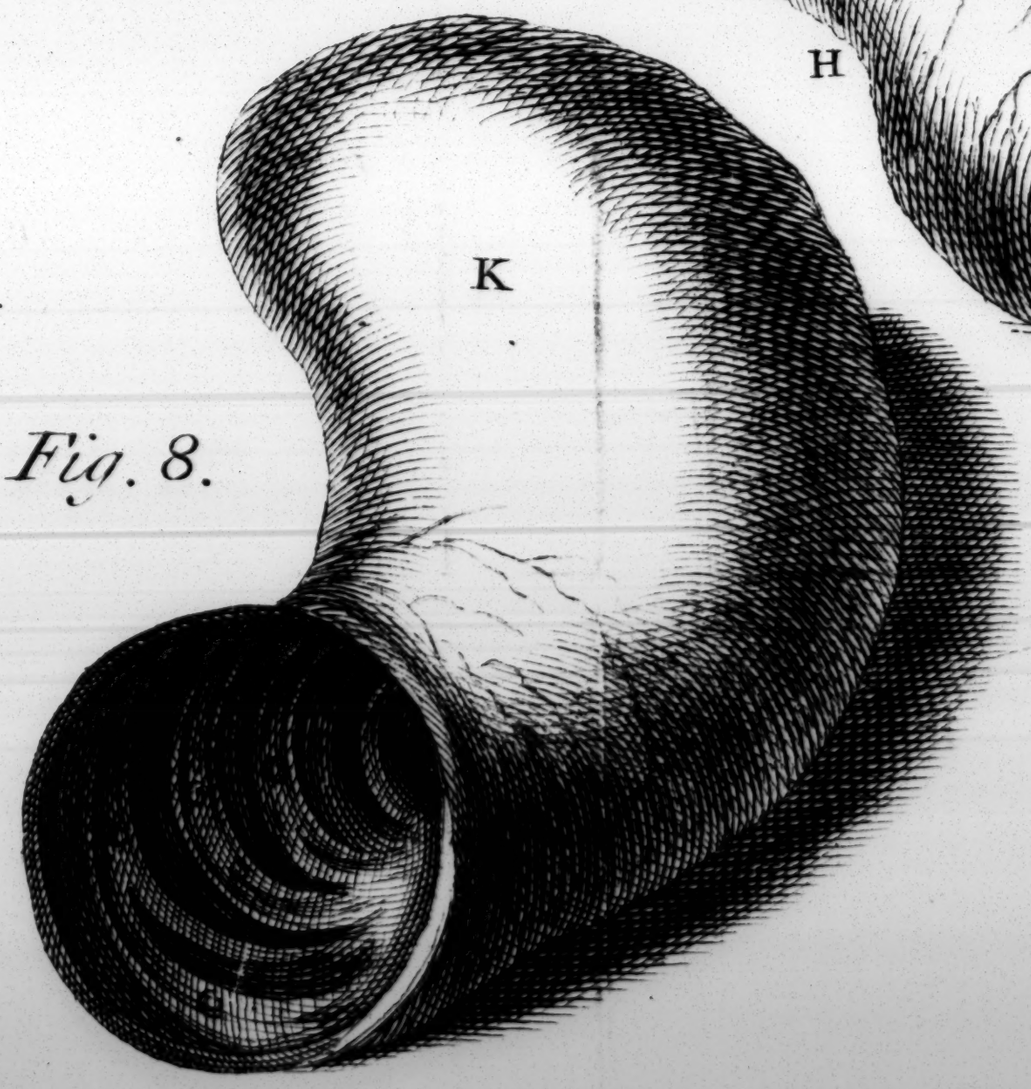


Fig. 8.



Hic ipſe autem fortior & craſſior eſt, quam in homine, & exterius atque inferius humeri latus occupat.

Inter flexores cubiti biceps ejuſdem eſt ſtructuræ uti in homine.

Brachialis verò interni origo ſeſe protendit ad capitulum uſque humeri, & in ſuo progreſſu hic muſculus totam humeri externam partem veluti faſciatim ambit, & eodem loco uti in homine inſeritur.

Flexores carpi adſunt tres ; Extenſores carpi tales ſunt quales in homine.

Extenſores digitorum ſunt primò communis, qui in tres digitos priores inſeritur ab humero ortus. Secundus cubito deſcendens, in ultimum & penultimum inſeritur.

Adeſt & indicator ſ. extenſor indicis.

Pronatores ſe habent uti in homine.

Ex ſupinatoribus longus brevior eſt brevi, cæterum ejuſdem ſitus uti in homine.

Aliorum muſculorum deſcriptiones reſervamus in aliud tempus.

Figurarum Explicatio.

Fig. 5. **A B.** Gula, **C D.** Duodenum, **E.** Ventriculus.

Fig. 6. **E H.** Ilei portio, **G. H.** Coli portio, **H F I.** Cœcum omnia magnitudine naturali.

Fig. 7. *a a* Coli valvula, prorſus fere uti depingitur in homine, niſi quod fermè ſit rhomboidalis ; *b c* Apertura Ilei in Colon.

Fig. 8. **K.** Portio Cœci, cujus ima pars eſt aperta versùs Colon, ut Valvulæ conniventes ramoſæ o o o in conſpectum veniant.

ERRATA.

Page 223. line 23. r. *Fig.* 2. ibid. lin. 31. r. *Fig.* 5.

FINIS.

Numb. 398.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *April*, *May*, and *June*, 1727.

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IV. *An Account of four Mock-Suns, ſeen at* Kenſington, March 1*ſt* 1726-7. *By Mr.* George Whiſton.

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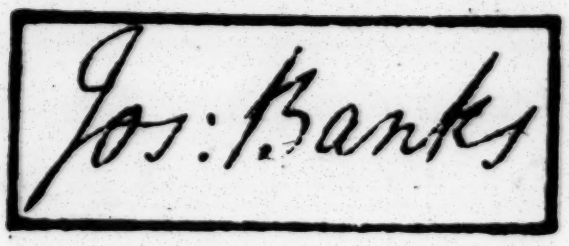


I. *Obſer-*

W.D.S.

Fig. 2.

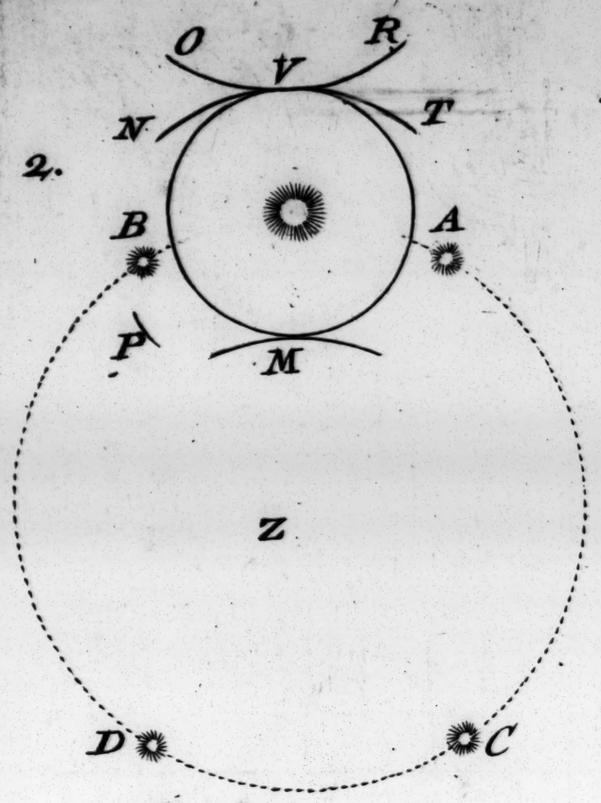


Fig. 1.

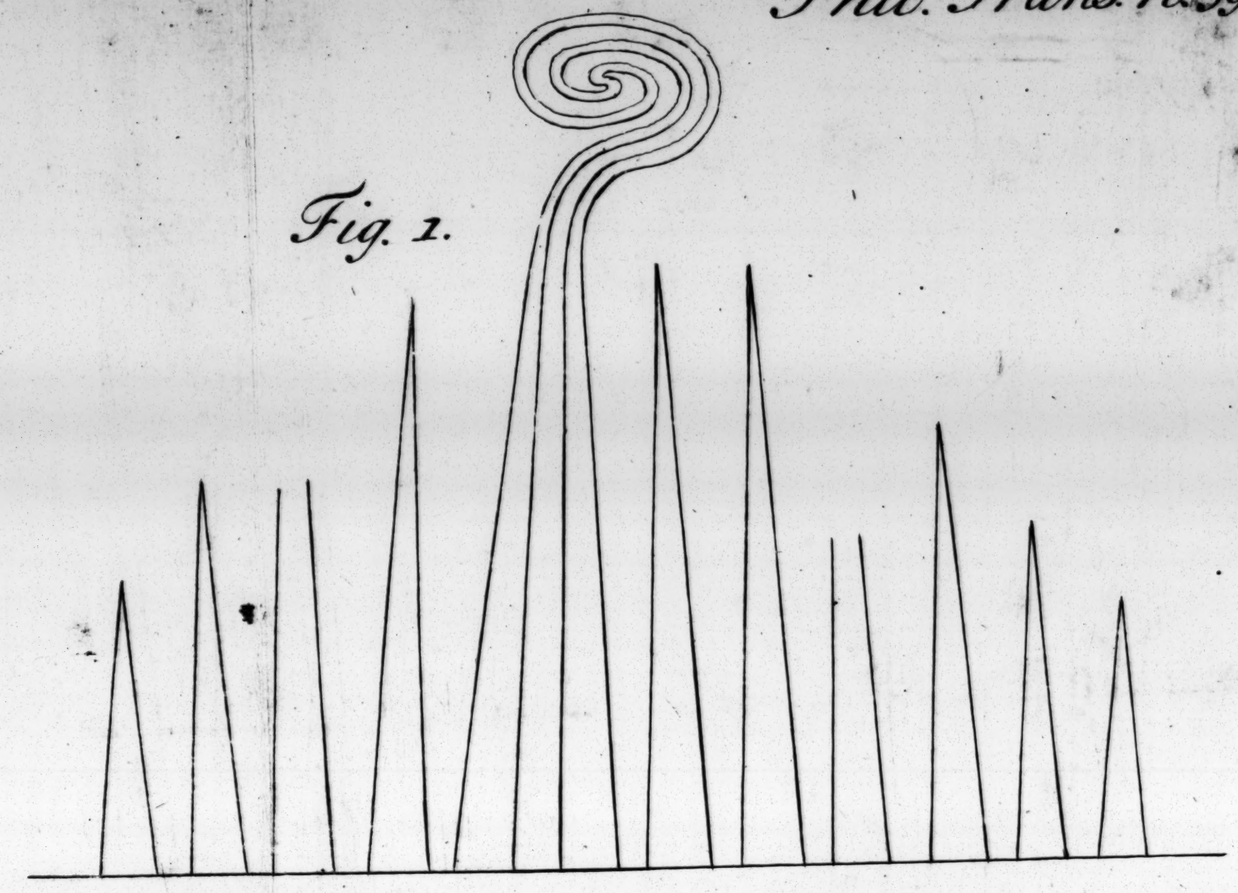


Fig. 3.

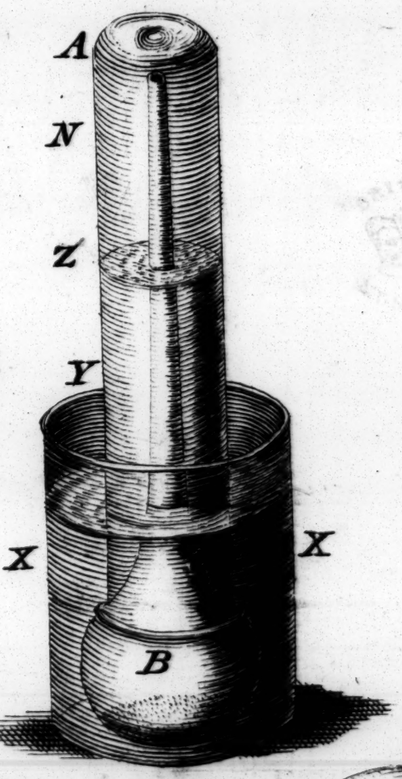
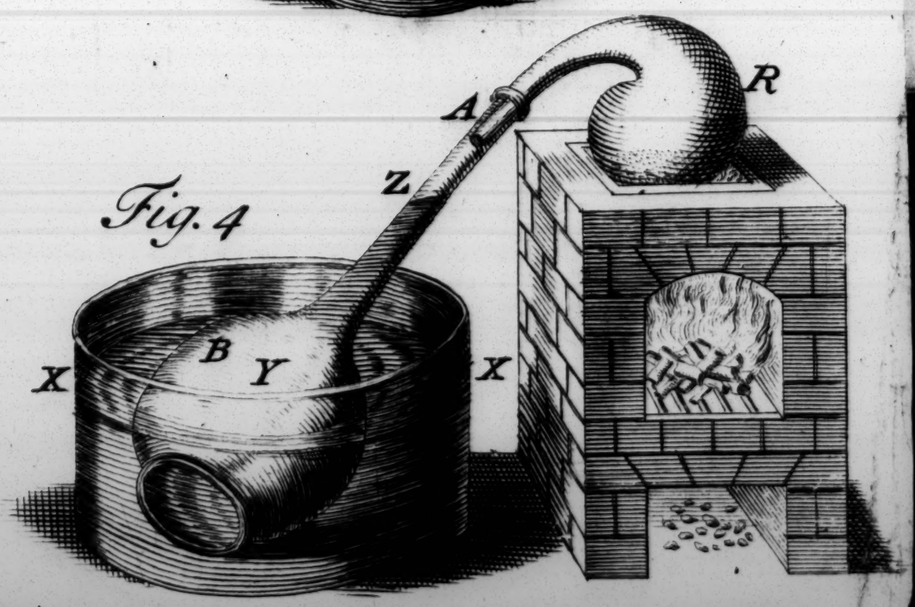


Fig. 5.

Fig. 4



The CONTENTS.

I. *Obſervations on the* Lumen Boreale, *or* Streaming *on* Oct. 8, 1726, *by the Reverend Mr.* W. Derham, *F. R. S.*

ALthough the *Lumen Boreale*, or *Streaming* hath been a common Phænomenon, ever ſince the ſurpriſing one, *Mar.* 6, 1715, 16, and as ſuch, would not deſerve to be taken notice of; yet in this of *Oct* 8, there were not only ſuch ſurpriſing, but different Circumſtances, that, I hope, the Royal Society will receive Accounts of them from many Hands; which may enable the skilful Meteorologiſts to find out their Cauſe.

For the clearer proceding in my Relation, I ſhall obſerve, that there are two ſorts of *Streamings*, which I have taken notice of; one, by way of *Exploſion* from the Horizon; the other, by *opening* and *ſhutting*, without ſuch Shootings up, and ſwift Dartings, as thoſe were which appeared in the *Lumen Boreale* before ſpoken of.

Of the latter ſort chiefly, was this laſt of *Oct.* 8: in which, altho' the *Streams*, or *Spires*, or *Lances*, or *Cones* (or, what ſhall I call them?) were as large and remarkable as in the former; yet they exhibited themſelves principally by the vaporous Matter opening and ſhutting, as if a Curtain had been drawn and withdrawn before them.

The firſt View I had of this Phænomenon, was preciſely at Eight o'Clock in the Evening: At which time, all I ſaw, was a long narrow *Faſcia*, like a white ragged Cloud, extended croſs the Heavens, from

W. b. S. to E. b. N. which in a few Minutes began to emit ſome Streams, and then diſappear'd: Which was ſucceeded by much Streaming in the Northerly Parts; and in a quarter of an Hour it began to reach other Points alſo; and ſoon after that, it ſtreamed all round in the Southerly, Eaſterly and Weſterly Parts as much, or nearly as much as in the North. Which was a thing I never had ſeen before in theſe Phænomena.

Fig. 1. Theſe *Streams*, or *Cones*, were for the moſt part pointed, ſo as to make the Appearance of *flaming Spires*, or *Pyramids*; and ſome others were truncated, and reach'd but half way: Some alſo were longer, and ſome ſhorter; ſome of which had their Points reaching up to the Zenith, or near it, where they formed a ſort of *Canopy*, or thin Cloud, ſometimes red, ſometimes browniſh, ſometimes blazing as if fired, and ſometimes emitting Streams all round it, which at that time gave it the appearance of ſuch a Star as our Knights of the Garter wear on their Breaſts.

This *Canopy* was manifeſtly formed by the Matter carried up by the Streaming on all parts of the Horizon: Which Matter ſometimes ſeem'd to aſcend with ſome force, as if impell'd by the Impetus of ſome exploſive Agent below, as I have ſaid it was in the Streaming of *March* 1715, 16, and which I gave the Society a large and particular Account of ſoon after. This forcible Aſcent of the ſtreaming Matter, gave a Motion to the Canopy, ſometimes a Gyration, like that of a Whirlwind; which was manifeſtly cauſed by the Streams ſtriking the outſide Parts of the Canopy, as in the Figure: But when the ſtreaming Matter hit the Canopy in the Middle, all was then in Confuſion.

Theſe two Particulars, namely, the *Streaming all round*, in all Points of the Horizon; and the *Canopy* in and near the Zenith, are what were taken notice of in all Parts of *England*, that I have met with any Accounts from; particularly, in *Northamptonſhire*, *Staffordſhire*, *Oxfordſhire*, *Wiltſhire*, *Berkſhire*, *Middleſex*, *Somerſetſhire*, and *Eſſex*, and in divers Parts beyond Sea.

The Reverend and Learned Mr. *Waſſe* gives me this Account of its Appearance at *Aynho* in *Northamptonſhire*, That at 7 h. 20′ p. M. he ſaw an Arch ſomewhat curved, like a Rainbow at firſt, and about half the Breadth of the Rainbow, and yellow; which in about ten Minutes began to twiſt, and make an Angle at the Zenith: That one End of it was pretty much to the Eaſt, and not directly to the North; and the Weſtern End deflected as much to the South: That it remained after the Twiſt, at the Zenith, without any great Motion, not a quarter of an Hour. After which, the Rods aroſe on all ſides, from the Horizon to the Zenith, the upper Points ſeeming to move thro' a ſort of Vortex quite out of our Atmoſphere: Which Rods, he thinks, roſe perpendicularly from the Horizon, but ſeemed to converge towards the Zenith, according to the Rules of Perſpective, by their Angle then being leſs than their Baſis at the Horizon: That a Redneſs was perceived, which, he thinks, was ſtrongeſt towards the Weſt; which Colour did not appear till the Arch brake into ſeveral Pieces, and overſpread the Heavens with a thin faintiſh Fire, thro' which they ſaw *Jupiter* very clearly.

This Account of Mr. *Waſſe*'s may ſhew, how the Phænomenon was in *England*; by reaſon moſt of the

Accounts I have met with, concur in the main with his. But in the more Southerly Parts of *Europe*, I take it to have been ſomewhat different. The News-Papers tell us from *Scaffhauſen*, that on the 19*th* of " *October* there was a great Alarm in many Parts of " *Switzerland*, on account of a great *Light* ſeen in " the Air, from 7 o'Clock till Midnight; which was " ſuppoſed to be the Reflection of ſome great Con- " flagration. At *Bern*, every body thought there " was a Fire in ſome part or other of the City or " Neighbourhood. At *Neufchatel*, the Alarm-Bells " were rung, and the Governour ſeveral Hours on " Horſeback, to give Orders, *&c.* as in Caſes of " Diſtreſs. All which they heard afterwards, was " only an *Aurora Borealis*." And from *Florence*, my ingenious and curious Friend Sir *Tho. Dereham* ſent me this Account, " As to the *Lumen Boreale*, which appear'd in " theſe Parts on *Oct.* 8*th* laſt, I ſaw it my ſelf in the " following manner: It was one Hour and half after " Sun-ſet, when I was paſſing thro' a Piazza in this " Town, that I diſcover'd the Phænomenon, that " ſeem'd one Mile long, and three quarters of a Mile " broad, of an almoſt perfect Oval Figure, hanging " North and North-Eaſt to us: The Edges of it were " of pale light Colour, like the firſt Dawn of the " Morning; and towards the Center, it encreas'd its " fiery Colour; ſo that in ſome Places it look'd as the " Fire of a Furnace; but in the very Center, and many " adjacent Parts, it was like a red-hot Iron growing " cold, that ſeems bloody. For a good while I could " perceive no Motion in it; but after a quarter of an " Hour, I diſcover'd a general ſlow Motion backwards " and forwards, like that one ſees of the Circulation " of the Blood in the Tails of Fiſhes, by the help of " the Microſcope, but no manner of Darting; inſo-

" much

" much that in another quarter of an Hour it vaniſh-
" ed imperceptibly, juſt as a Rainbow, and the Air
" grew dark again, that was ſo luminous before, that
" one might read a Manuſcript by the ſame. It is
" very remarkable, that at *Fieſole*, a Town with-
" in a ſhort Mile of this, the Phænomenon ſeem'd to
" thoſe Inhabitants, to be between them and us, and
" they thought our Town was burning: Whereby it
" appears not to have been very deep, nor very high;
" *Fieſole* ſtanding upon a Hill half a Mile high, and
" to the North-North-Eaſt of this Town.

To theſe Obſervations of my ſelf, and ſome of my Correſpondents, I ſhall add two or three things more, before I enquire into the Cauſe of the Phænomena.

One thing that was taken notice of in moſt Places, was, That in ſome part of the greateſt Streaming, the Vapours between the Spires, or Lances, were of a Blood-red Colour. That which I obſerv'd, was, That about half an Hour after Eight o'Clock, the Vapours towards the South-Weſt were very denſe, and for ſome time red. And not long after, the like Redneſs aroſe in the North-Eaſt, and the other gradually went off. Both which gave thoſe Parts of the Atmoſphere the Appearance of blazing Lances, and bloody-colour'd Pillars.

Another thing I took notice of, was, a ſtrange Commotion, and Working among the Streams, as if ſome large Cloud, or other Body was moving behind them, and diſturb'd them.

In the Northerly and Southerly Parts the Streams were perpendicular to the Horizon; but in the intermediate Points they ſeem'd to decline more or leſs one way or other; or rather to incline towards the Meridian.

As

As for the *Weather*, the preceding Day was cloudy, with an Hoar-Froſt in the Morning; but it clear'd up, and grew warmer afterwards; but towards the Horizon, very vaporous. And the next Morning (after the Streaming) before Sun-riſing, the Air was full of Vapours, with divers thin vaporous Clouds, ſome of a lucid brown, ſome reddiſh, which I took to be Remains of the Streaming, which, I was informed, continued all Night.

As for the Cauſe of theſe Phænomena, I take it to be from the ſame Matter, or Vapours, which produce Earthquakes: And that for theſe Reaſons: *Firſt*, Becauſe ſome of theſe Phænomena have been followed by Earthquakes As that which *Stow* gives an Account of in his *Annals*, in the Year 1574, on *Nov.* 14. in which he ſaith, *were ſeen in the Air ſtrange Impreſſions of Fire and Smoak to proceed forth of a black Cloud in the North towards the South.* That *the next Night following, the Heavens from all parts did ſeem to burn marvellous ragingly, and over our Heads the Flames from the Horizon round about riſing did meet, and there double and roll one in another, as if it had been in a clear Furnace.*

And after this (he tells us) *followed on the* 26th *of* February, *great Earthquakes in the Cities of* York, Worceſter, Glouceſter, Briſtol, Hereford, *and in the Countries about, which cauſed the People to run out of their Houſes, for fear they ſhould have fallen on their Heads. In* Tewksbury, Breedon, *&c. the Diſhes fell from the Cupboards, and the Books in Mens Studies from the Shelves:* With more to the ſame purpoſe.

So this laſt (in *October*) was preceded by that fatal Earthquake at *Palermo* in *Sicily*, and ſucceeded by one in *England*, on *Tueſday*, *October* 25, following. This, I hear, was perceived in *London*, and was very conſiderable

derable at *Dorcheſter*, *Weymouth*, *Portland*, *Portſmouth*, *Purbeck*, and divers other Places in *Dorſetſhire*, that it cauſed the Doors to fly open, ſhook down Pewter off the Shelves, and was felt in ſome Ships that lay in the Harbours.

2. Another Reaſon is, That I am aſſur'd by an ingenious ſenſible Gentleman of my Acquaintance, that as he was viewing this Appearance, on the Top of his Houſe at *Little Chelſea*, he plainly perceived a Sulphureous Smell in the Air; and that another Perſon did the ſame, on the Top of another Houſe near him.

3. Another thing which concurs with what hath been ſaid, is, That I am aſſured from ſeveral Perſons, that an Hiſſing, and in ſome Places a crackling Noiſe was heard in the time of the Streaming, like to what is reported to be often heard in Earthquakes.

And now, for a Concluſion, I ſhall remark two things upon what hath beed ſaid.

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1. That it may help the ſagacious Meteorologiſt to reſolve ſeveral Difficulties relating to theſe *Northern Lights*, to obſerve, that what was *Streaming* or *Darting* in our Northern Parts, was only a remarkable Light, or Blaze in *Italy*, and the Southern Parts, if I take Sir *T. Dereham*'s and the News-Papers Accounts right.

2. If thoſe Phænomena have the ſame Origine that Earthquakes have, that then they are, doubtleſs, of great uſe to the Peace and Safety of the Earrh, by venting ſome of that pernicious Vapour and Ferment

that

that is the Cauſe of thoſe terrible Convulſions, which Earthquakes are accompanied with.

I forgot (when I deſcribed the *Canopy* or *Corona*) to ſay, that it did not reſt in one place; but changed its Poſition, ſometimes higher near the Zenith, and then towards the Eaſt, and South-Eaſt, 10 or 15 Degrees, and then back again nearer the Zenith, according as the darting Matter directed it: But I do not remember, that this Canopy was at any time directed towards the Weſtern Points.

In moſt of the *Northern Lights* that I have ſeen, there generally was a dark Bank of Vapours, circular on the Top; but whether this of *Oct.* 8, had any ſuch Arch, I could not ſee at *Upminſter*, where I was ſurrounded with Trees.

II. *The*

II. *The ſame obſerv'd at* Southwick *in* Northamptonſhire. *By* George Lynn, *Eſq;*

	Barom. Alt.	*Therm*	*Wind*	
Oct. 8, 10, Morn.	29. 90	54.	W. 1.	Weather fair and clear.

THIS Evening appear'd an *Aurora Borealis*, I think, full as remarkable as that in *Mar.* 1716, tho' varying in Form: It began about Six at Night to be light in the North, with Streaks proceeding from it, and ſpread gradually both towards the Eaſt and Weſt, the South being ſtill very clear; but before Seven it left all the Northern Parts (except towards the Zenith) and cover'd all the Southern. Soon after which, there appear'd a white Arch proceeding from Eaſt to Weſt, paſſing near the Zenith, but more South, which ſeemed fix'd for a time; but about 10 Minutes paſt Seven was diſperſed, and immediately ſucceeded by a kind of Glory of an Oval Form, the longer Axis from Eaſt to Weſt, ſomething South of the Zenith, with Rays ſhooting up from all parts, and interchanging ſwiftly, for about 15 or 20 Degrees from it; the reſt of the Heavens (except the North, which ſtill continued very clear) affording various Phænomena. In the Eaſt there was a quick Succeſſion of Columns of the *Iris* Colours, inclinable to White, the Weſt to Purple, and about the South-Weſt, for a good ſpace, appeared almoſt a blood red Coruſcation, which continued 5 or 6 Minutes.

Theſe Appearances in a quarter of an Hour became leſs remarkable; though the *Aurora* continued moſt of the Night, and afforded a Light generally equal to the Moon in its Quadratures. Looking with my Teleſcope at *Jupiter*, I found both his Satellites and Belts appear as plain thro' the *Aurora*, as if the Sky had been perfectly clear.

III. *Regiſtrum Obſervationum de Lumine Boreali per quadriennium. Ex Epiſtola Viri Cl.* Gulielmi Raſtrick, *ad* Martinum Folkes, *Arm. R. S. V. Pr.* Lennæ Regis, Martii 20, $172\frac{6}{7}$.

MItto tibi, Vir Ampliſſime, Regiſtrum breve Obſervationum de *Luminibus Borealibus*, per quadriennium alterum (videlicet ab anno 1722.) à me continuatum. Neſcio profectò an in ulla re dignum ſit tua cognitione; ſed qualecunque eſt, judicio tuo & candori refero. Sum

&c.

GULIELMUS RASTRICK.

Die Martis, *Feb.* 19, 1722,3, hora ſeſquidecima.

Die Veneris, *Martii* 15, 1722,3, ab hora octava, ad mediam uſque noctem.

Die Martis, *Aug.* 20, 1723.

Die Dominica, *Oct.* 20, 1723, ab hora ſexta, ad mediam noctem.

Die Dominica, *Sept.* 26, 1725, Aurora Borealis viſa eſt ab hora ſeptima, ad horam decimam, cum Radiationibus variis.

Die Lunæ, *Oct.* 3, 1726, per totam noctem.

Item Die Martis, *Oct.* 4, 1726.

Itemque Die Saturni, *Oct.* 8, 1726, per totam noctem. Mirum ſanè Phænomenon, de quo nihil dixi, quia tam accuratè deſcribitur à Dominis *Langwith*, *Huxham*, *Hallet*, *Hadley*, & *Johan. Ludov. Calandrino*, in *Actis Londinenſibus*, N°. 395.

Die Mercurii, *Oct.* 26, 1726, circa horam decimam.

Die Veneris, *Martii* 3, 1726,7, ab hora octava, ad mediam noctem. Aurora hac nocte (ut mihi videbatur) longè mirabilior fuit illâ *Oct.* 8; & credo equidem, nullatenus diſcrepavit (ſecundum deſcriptiones quas habemus) ab illo memorabili Phænomeno, ſexto *Martii*, 1715,16.

Item Die Domin. *Martii* 5, 1726,7.

IV. *An Account of four Mock-Suns, ſeen at* Kenſington, March *1ſt* 1726-7. *By Mr.* George Whiſton.

ON Wedneſday, March 1ſt, 1726-7, walking in a Garden at *Kenſington*, about a quarter after Ten, I happen'd to obſerve the following Appearance, which being very rare with regard to the whole of it, ſeems to deſerve that ſome Account ſhould be given of it.

I at firſt took notice of the Halo about the Sun, V. M. (Fig. 2.) with its uſual Circumſtances, which are pretty frequent; the upper part of it was very luminous, having a confus'd mixture of the Rainbow-Colours in it, and being touch'd at the Vertex with the two other Curvatures, OVR, NVT, in the Situation which the Scheme ſhews; tho' the latter Arch NVT, did not appear till ſome time after. The Bottom part of it alſo at M, which appeard a little above the Horizon, had ſomething of the ſame nature, but not in ſo great a degree.

I preceiv'd, preſently likewiſe, the two *Parhelia*, A, B; whoſe Diameters were pretty large, and whoſe Brightneſs and Colour was pretty much as the upper part of the Halo.

As the Halo was at that time not quite perfect, but had ſome parts interrupted, I thought that the two *Parhelia* were in the Circumference of its Circle, as uſual; but after about a quarter of an hour, I directly obſerv'd the Halo to paſs between the *Parhelion* A, and the true Sun; and I have no reaſon to doubt the

ſame

ſame of the other B, alſo, tho' I don't remember that I directly obſerv'd that.

The *Parhelia* A, B, therefore, which were but a little diſtant from the Circumference of the Halo, began now to appear with narrow, pale, whitiſh Streaks of Light, in the nature of Tails, proceeding from them; but ſoon extended themſelves ſo far, that they met in the Point oppoſite to the Sun, and form'd the Great Circle, A B C D, parallel to the Horizon, whoſe Breadth was about half that of the Halo.

Upon viewing it carefully all round, I ſoon diſcover'd a third Mock-Sun, C, of a plain whitiſh Light, without any mixture of Colours, (which was alſo the Caſe of the whole great Circle,) and preſently alſo a fourth, D, both of them pretty exactly reſembling each other, (as the two firſt did themſelves likewiſe,) very much inferior to the *Parhelia* A, B, in Brightneſs, tho' not ſo much in magnitude; for I eſtimate their Diameters to have been to the two firſt *Parhelia*, as 4 to 5.

As I had no opportunity of meaſuring the ſeveral Angles, I have plac'd the Mock-Suns, C, D, in the Scheme, rather in Agreement with former Obſervations, than my own Gueſſes; for they appear'd to me to be at a greater diſtance from each other, and nearer reſpectively to the two firſt *Parhelia*, which Difference M. *Huygens* attributes to the different Altitude of the Sun.

The Arch, NVT, not being very viſible while the Great Circle was, and indeed not extending it ſelf at any time near ſo far as to the *Parhelia*, or the Circumference of the great Circle, I could not determine by a direct Obſervation, whether the *Parhelia* A, B, appear'd in the Interſection of that Circle produc'd, with the great Circle; but the Curvature appear'd to me ſo plainly different from that, its Center not being,

I reckon,

I reckon, above M, that I cannot but believe the *Parhelia*, A, B, were neither in the Interſection of N V O, with the great Circle, A B C D, nor of the Halo with the ſame Circle, in one of which Circumſtances they have hitherto appear'd; but between thoſe two Points, and much nearer to the Circumference of the Halo.

I thought I ſaw plainly at one time likewiſe, a ſmall Portion of a Secondary Halo, if I may ſo call it, as in the Scheme at P. It ſeem'd evidently to be an Arch of a Circle concentrical with the Halo, and ting'd with the Rainbow-Colours, whoſe Diameters might perhaps be to that of the Halo, as 4 to 3; but as it appear'd but for a little time, I would not be thought poſitive about it.

I don't at all remember, that during the time I watch'd it, I ever obſerv'd the great Circle A B C D, to be viſible within the Halo, between A and B, tho' all the other part of it was ſometimes very perfect.

This Face of the Heavens continued, tho' with an Interruption of ſome parts now and then, till about a quarter after eleven, when I left it, and could not return till about twelve, at which time the Sky was clouded over, (which had been before only hazy, a ſure Criterion of theſe Appearances) and this Phænomenon no longer viſible.

V. *Chry-*

V. *Cryſtallorum quarundam rariſſimarum nuper detectarum deſcriptio, ex literis Viri Cl.* Johannis Jacobi Scheuczer, *ad Dominum* Hans Sloane, *Bar. Reg. Soc. & Coll. Med. Lond. Præſidem.*

IN ſummis *Grimſulæ* jugis, paucis abhinc annis detecta eſt, nunc exhauſta, in ſaxo medio, cryſtallorum vena, quibus nec majores, nec puriores, vidit fortè orbis. Maxima earum pars, ad 60 circiter centenarios, in manibus eſt *D. Friſching* paſtoris *Haſleani*, venales omnes, ſed pretio, quod ex ſequentibus patebit, haud vulgari, quod fortè, præter puritatem, excuſat rara magnitudo. Præcipuarum en, quam nuper ab ipſo expetii poſſeſſore ſeriem!

Num. 1. Duorum vel $2\frac{1}{2}$ centenariorum, longa 2 ped. $9\frac{1}{2}$ dig. Peripheria 3 ped. $7\frac{1}{2}$ dig. limpidiſſima, hexagona. Pretium libræ $1\frac{1}{2}$ Ludov. aur.

N. 2. Pondus 136 ℔, longit. 2 ped. 3 dig Peripheria 2 ped. 9 dig. Purpuras quaſdam habet ad marginem, puriſſima cæterùm. Pretium libræ 1 Ludov. aur.

N. 3. Pondus 135 ℔, longit. 2 ped. 4 dig. Peripheria 3 ped. 2 dig. limpidiſſima, excepto apice; pretium libræ 4 Floren.

N. 4. Pondus 96 ℔, longit. 2 ped. Peripheria 2 ped. 9 dig. pretium idem.

Ita deſcendendo pergit ad 10 librarum pondus, qualitatis, proportionis, pretii varii.

VI. *An*

VI. *An Account of a Stone taken out of a Horſe, at* Boſton *in* New England, *in the Year* 1724. *By the Honourable* Paul Dudley, *Eſq*; *F. R. S.*

THE Owner of the Horſe never perceived that he ail'd any thing, till within a few Days before he dyed, and then ſuſpected that he might be troubled with the Gravel or Stone, by the great Pain the Horſe ſeem'd to be in, when he ſtaled or dung'd; for he would groan and ſweat prodigiouſly. Upon which he got a Farrier, who applied ſomething to break the Stone; but in a very ſhort time the Horſe dyed, and the Farrier, being ſomewhat curious, was reſolv'd to open him, and in the great Paunch, found a Stone of five Pounds and an half Weight, almoſt as round as a Globe; for it meaſured ſeventeen Inches round one way, and ſeventeen Inches and three quarters the other. The Grit was like your *New-Caſtle* Grindſtone; but was worn ſmooth in the Horſe's Stomach, the Colour ſomewhat like that of a Nutmeg, but more of the ordinary Millſtone. I could not perſwade the Owner to break it; but by the lightneſs of it, conſidering its Bulk, I am apt to think it might be porous within. How long this Stone was generating or what produced it is altogether uncertain. The Owner of it was a common Carter to a Griſt-Mill; and ſome have thought that the Horſe might either in his Provender out of the Mill or by licking of Mill-Stones that ſometimes ſtand up by the ſide of the Mill get the firſt Seed of this Stone into his Stomach. The Weight of the Stone at length made a Fracture in the Paunch,

which proved his Death : For before the Breach, and while the Stone roll'd in his Stomach, he was very well.

The largeſt Stone found in any Animal that the *Philoſophical Tranſactions* give an account of, weighed but four Pounds, four Ounces.

VII. *An Account of a* Polypus *cough'd up from the Windpipe ; in a Letter from the Ingenious Dr.* Samber, *Phyſician at* Salisbury, *to Dr.* Jurin, *Secr. R. S.*

THE 15th of laſt *December* at 10 at Night, I was ſent for to one Mr. *Tompſon*, an Officer of the Exciſe in this Town, who was taken with ſo violent a Flux of Blood, that in a ſhort ſpace of time he loſt near three pounds of Blood, as near as we could judge : By the time I came, it was pretty well over; only he ſeem'd to have ſomething, when he cough'd, that ſtuck in the Paſſage, which he could not get up, and by its rattling I thought it very looſe. I order'd what I thought proper in ſuch a caſe, and left him : Next Morning they told me, that half an hour after I was gone, he had cough'd up what they ſhew'd me on a Sheet of Paper, which Mr *Gifford* the Apothecary (a very honeſt Man, and very ingenious in his Buſineſs) had deſir'd them to keep till I came. Upon putting it into Water, I found it a *Polypus* ; and, as I think, a very remarkable one. I here ſend you a draught of it *Fig.* 3. exactly done by a Painter ſince dead. I could find by my Blow-pipe, that it was hollow ; but its being torn off with ſuch violence, has made ſo many

many Holes in it, that it cannot be blown up. You will (I believe) conclude, it lin'd the *Bronchia*, and that the Air had a Paſſage through it, and that a violent Fit of coughing had ſeparated the Adhæſion, and brought on that violent Flux of Blood, &c. He had been tormented with a Cough for more than ſix Months, was a gouty Man; but after this was cough'd up, and ſo large an Ulcer made, he had all the ſucceſſive Symptoms of a fatal Conſumption; as Cough, Spitting, Hectick, colliquative Sweats, Diarrhæa, and the 16*th* of this Month died, Aged near 50.

Sarum, Jan. 28, 1726-7.

VIII. *An Account of a Book entitul'd* Vegetable Staticks: *Or an Account of ſome* Statical Experiments *on the* Sap *in* Vegetables; *being an* Eſſay *towards a* natural Hiſtory of Vegetation. *Alſo, a Specimen of an* Attempt *to Analyſe the* Air, *by a great Variety of* Chymico-Statical Experiments; *which were read at ſeveral Meetings before the* Royal Society, *&c. By* Stephen Hales, *B.D.F.R.S. Rector of* Farringdon, Hampſhire, *and Miniſter of* Teddington, Middleſex. *The Account by the Rev.* John Theoph. Deſaguliers, L.L.D. *R.S.S.*

AS the Antients us'd to ſay, that Geometry and Arithmetick are the Wings of a Mathematician; ſo a Mechanical Hand, and a Mathematical Head are the neceſſary Qualifications of an Experimental Philoſopher. The firſt alone may enable a Man to make a great many Experiments, but not to judge of them: For without being able to obſerve, compare, and calculate the exact Quantity of Weight, Force, Velocity, Motion, or any other Change to be taken notice of in making Experiments; Effects may be attributed to Cauſes which are not adequate to them, and ſometimes expected to be produc'd even without a Cauſe; as appears by the Coſt and Trouble of thoſe who have attempted to find

find the Perpetual Motion. Tho' ſuch Perſons may make ſome Diſcoveries, their Philoſophy will be at beſt but conjectural, and their Concluſions only gueſs-work. The mere Mathematician on the other hand, wants *Poſtulata* in Phyſics; or taking things for granted upon the Reports of others, comes often to wrong Concluſions, tho' he reaſons juſtly; becauſe his Premiſes are falſe: Whereas he might have been ſet right in ſeveral caſes, by only obſerving the Operations of Handycraft-Workmen, which he had over look'd or deſpis'd. Men of warm Imaginations, who wanted Mechanicks or Mathematicks, or wou'd not apply them to Phyſicks, have peſter'd the Learned World with Philoſophical Romances, ſuch as the Carteſian Syſtem, contriv'd for the Diverſion of the lazy and talkative; or Modern Theories, which their Authors, blinded by Vanity, and urg'd by Obſtinacy, have defended to the laſt, againſt Reaſon, Mathematicks and Experiments; nay, and have often for the Support of their Hypotheſes, contriv'd complex Experiments, from whoſe Inaccuracy they have drawn Arguments to deceive themſelves and others, as much to the diſadvantage of true Philoſophy, as the Scholaſtick Diſputes of the *Ariſtotelians*; in which, Victory, not Truth, was contended for.

But the incomparable Sir *Iſaac Newton* has not only ſhorten'd the Geometrician's Work, by his wonderful Diſcoveries in abſtract Mathematicks; but has alſo taught us, by his own Practice, how to make, and judge of, Experiments and Obſervations with the utmoſt Accuracy: And as he avoided making Hypotheſes; he was ſo cautious as to deliver only by way of Queries, ſeveral Truths which he was convinc'd of; becauſe he wanted a ſufficient Number of Experiments to make them as evident as thoſe others, whereby he has ſo

far

far improv'd and advanc'd Natural Knowledge. Our Author has follow'd his ſteps, aſſerting nothing but what is evidently deduc'd from thoſe Experiments, which he has carefully made, and faithfully related; giving an exact Account of the Weights, Meaſures, Powers and Velocities, and other Circumſtances of the Things he obſerv'd; with ſo plain a Deſcription of his *Apparatus*, and manner of making every Experiment and Obſervation, that as his Conſequences are juſtly and eaſily drawn, ſo his Premiſes or Facts may be judg'd of by any Body that will be at the Pains to make the Experiments, which are moſt of them very eaſy and ſimple.

His Account of every Thing is written in ſuch an intelligible manner, that the inquiſitive Reader is capable of underſtanding it, without being puzzled with perplex'd Calculations and complex Experiments; which Authors have ſometimes contriv'd, in order to be admir'd for thoſe Things, which they themſelves found out either by mere chance, or with very little Labour. He has illuſtrated, and put paſt all Doubt, ſeveral Truths mention'd in Sir *Iſaac Newton's* Queries; which tho' believ'd by ſome of our Eminent Philoſophers, were call'd in queſtion by others of an inferiour Claſs, who were not acquainted with thoſe Facts and Experiments upon which Sir *Iſaac Newton* bad built thoſe Queries.

But not to detain my Reader too long in general Remarks, I proceed to give a ſhort Account of each Chapter of our ingenious and indefatigable Philoſopher's Book.

CHAP.

CHAP. I.

Experiments, ſhewing the Quantity of Moiſture imbib'd and perſpir'd by Plants and Trees.

OUR Author having cover'd with mill'd Lead a Garden-Pot, in which a Sun-flower was growing, ſo as to leave only one Paſſage for Air to communicate with the Earth, and another to pour in Water to water the Plant, made ſeveral curious Experiments upon it; from the Obſervation of which it appear'd,

1. That the Plant, which weigh'd about 3 lb, perſpir'd 30 Ounces in a 12 Hours Day, in the Month of *July*, 1724. But in a warm Night it perſpir'd only three Ounces, and nothing in a cold Night; nay, ſometimes it gain'd Weight by imbibing Dew.

2. That as the Area of the Surface of the Leaves was equal to 5616 ſquare Inches, and the Area of the Roots only to 2286 ſquare Inches; the Moiſture or Water imbib'd by the Roots to ſupply the Perſpiration at the Leaves muſt move faſter in the Roots than thro' Leaves, in the Proportion of 5 to 2; but in the Stem, whoſe tranſverſe Section was one ſquare Inch, faſter than in the Leaves, in the Proportion of 5616 to 1.

3. That by comparing his Experiments made on this and other Sun-flowers and Plants, with Dr. *Keil's* Experiments mention'd in the *Medicina Statica Britannica*, it is evident, that a Man (Surface for Surface) perſpires more than a Plant, in the Proportion of 3 ½ to 1 — That the Quantity of the Food of a Man is to the Food of a Plant nearly as 7 to 2; but, Bulk for Bulk, the Plant imbibes into its Veſſels 17 times

times more Fluid than the Quantity of the Chyle which enters into a Man's Veſſels; and that becauſe, as the Sap to Plants is leſs nutritive than Chyle to Man, it muſt be more in Quantity. To ſuck in the more Sap, a Plant has a very great Surface, and thereby (Perſpiration promoting its imbibing of the Sap) perſpires 17 times more than a Man in proportion to their reſpective Weights.

4. That a Man or a Plant may perſpire different Quantities, and yet continue in a healthy ſtate. A Man may perſpire in 24 Hours from 1½ ℔ to 3 ℔; and a Plant, which at one time perſpir'd but 16 Ounces, may without being leſs healthy, perſpire 28 Ounces in the ſame time.

Mr. *Hales* obſerv'd, that in order to ſupply the Evaporation in the Leaves of a Vine, the Sap roſe 152 Inches in 12 Hours, ſuppoſing the Sap to riſe as denſe as Water; but if it riſes in a Steam, which muſt at leaſt be 10 times rarer than Water, then the Velocity of the Sap will be 10 times greater, and conſequently riſe at the rate of 125 Inches in an Hour.

N.B. *That the Heat of the Sun ſhould rariſie the Moiſture of the Earth to that Degree, or a great deal more, to drive it into the Roots of Plants, is very probable from ſome Obſervations that Mr.* H. Beighton F.R.S. *and my ſelf have made upon the Engine to raiſe Water by Fire; whereby it appears that the Steam or Vapour of boyling Water is rarer than the Water from which it was produc'd, above* 13000 *times, when its Elaſticity is equal to that of common Air.*

Our Curious Experimenter, by his Obſervations upon 12 Evergreens, found, that they perſpire much leſs than other Plants, and compares them to the exanguious Tribe of Animals; which, as they perſpire little,

little, ſo they live the whole Winter without Food. He likewiſe obſerv'd, what part of an Inch in Thickneſs is perſpir'd by a Man and by ſeveral Plants: By a Man in 24 Hours $\frac{1}{50}$ part of an Inch; and by the following Plants, *viz.* the Vine, the Sun-flower, the Cabbage, the Apple-Tree, the Lemon-Tree, $\frac{1}{191}$, $\frac{1}{165}$, $\frac{1}{86}$, $\frac{1}{104}$, $\frac{1}{242}$, part of an Inch in 12 Hours.

That when the perſpiring Surface is diminiſh'd by taking away the Leaves, the power of imbibing is likewiſe diminiſh'd; for the ſame Bough, which with the Leaves on imbib'd 30 Ounces in 12 Hours, without the Leaves imbib'd only 1 Ounce.

That Fruit will imbibe thro' the Stem in proportion to its Surface, tried in an Apple, which imbib'd as much as two Leaves equal to it in Surface.

That as the Leaves draw Nouriſhment to the Bloſſoms and Fruit, ſo they grow large firſt in thoſe Places where the Bloſſoms are.

That Hops in the middle of an Hop-Ground thrive beſt, becauſe they imbibe and perſpire the leaſt.

That by 9000 Hop-Vines growing on one Acre of Ground, 220 Gallons of Water are imbib'd in a 12 Hours Day, which Quantity of Water is equal to a Depth of $\frac{1}{101}$ part of an Inch ſpread on the ſaid Surface.

That Fire-Blaſts (as the Gardiners call them) may be occaſion'd by Solar Rays reflected from, or condens'd by Clouds, or even collected by means of the denſe Steams, which ariſe moſt plentifully in the middle of the Hop-Ground.

That the perſpiring Matter of Trees is not protruded by the Power of the Veſſels, but exhal'd by Heat, which he proves by ſeveral curious Experiments.

He made Trees imbibe Spirit of Wine, and other Liquors impregnated with Aromaticks, which gave a Smell and Taſte to the Stalk, Leaves and Wood; but could not penetrate to the Fruit, by reaſon of the Fineneſs of the Capillary Sap-Veſſels near the Fruit, which aſſimilated to their own Subſtance the high-taſted and perfum'd Liquors. Spirits kill'd the Trees, as far as they were imbib'd; but ſcented Waters did not.

He receiv'd in proper Veſſels the Matter perſpir'd from Trees which is a taſteleſs Water; but corrupts and ſtinks ſooner than common Water.

He weigh'd three Cubick Feet of the ſame ſort of (Brick-) Earth, taken from the Surface of the Earth downwards, in *July* 1724, the lighteſt of which was about twice the ſpecifick Gravity of Water: Then ſetting them to dry, found by the loſs of Weight, how much Water they contain'd; the greateſt Quantity exhal'd from a Cubick Foot being 10 ℔, and the leaſt, 6 ℔, 11 Ounces. N.B. *The Weights he made uſe of were Avoirdupoids.*

In order to find the Heat of the external Air in Green-Houſes and in Stoves, as alſo the Degrees of Heat at ſeveral Depths in the Earth, and in hot Beds, he provided ſix Thermometers, whoſe Stems were of different lengths, but adjuſted in their mark'd Degrees; ſo that they began at the freezing Point, and ended at 90 Degrees, where the Heat was equal to the Heat of warm Water, that may be born by the Hand, without ſtirring it about, which is the middle Point between freezing, and the Heat of boiling Water.

By theſe Thermometers he obſerv'd the Degrees of Heat in ſeveral Caſes to be as follows: *Of the Blood* =64. *Of the Human Body* =54. *Milk from the Cow, and Heat fit to hatch Eggs* = 55. *Urine* = 58. *Temperate*

perate Air = 18. *Greateſt Heat of Sunſhine in the Year* 1724 = 74. *And now this Year* 1727, *it is at* 76. *The common Heat of the Noon-Sun in* July = 50. *Air in the Shade at the ſame time* = 38. *The Heat in* May *or* June, *the fitteſt for Plants,* = *from* 17 *to* 30 *Deg. Autumnal Heat* = *from* 10 *to* 20. *A hot Bed, too hot for Plants, and about the Heat of Blood in Fevers* = 75. *Due Heat of a hot Bed* = 56. *Heat under a Glaſs at the ſame time* = 34; *and in the open Air* = 17.

Thermometers of this kind are now made by JOHN FOWLER in *Swithin's Alley* near the *Royal Exchange*, which have the Names of the following Plants, oppoſite to their reſpective moſt kindly Degrees of Heat, *viz. Melon-Thiſtle* = 31, *Ananas* = 29, *Piemento* = 26, *Euphorbium* = 24, *Cereus* = 21 $\frac{1}{2}$, *Aloes* = 19, *Indian Fig* = 16 $\frac{1}{2}$, *Ficoides* = 14, *Oranges* = 12, *Myrtles* = 9.

He obſerv'd the Air to be cooler than the Earth out of the Sun, but hotter in Sunſhine; and that as ſoon as the Heat of the Sun is ſo far decreas'd, as no longer to give a brisk Agitation to (or turn into Steam) the Moiſture of the Earth, the Leaves of the Plants fall off.

CHAP. II.

Experiments whereby to find the Force with which Trees imbibe Moiſture.

OUR Author, in order to make a juſt Eſtimate of the Force with which the Moiſture is drawn up by Plants and Trees, contriv'd an *Aqueo-mercurial* Gage in the following manner. He took a Glaſs-Tube of an Inch Diameter, and about 8 Inches long, into one End of which he cemented a ſmaller Tube of about $\frac{1}{4}$ of an Inch Diameter, and 18 Inches long; into the other End he, ſucceſſively, cemented a Root, or Branch, or Stem of a Tree or Plant, whoſe imbibing Force he wanted to find out, which he did in the following manner, *viz.* He turn'd the ſmall End of the Gage upwards; and having pour'd Water into it, down upon the Wood cemented in, he ſtopt the Orifice with his Finger; then turning the little End downward again, he plac'd it into a Veſſel of *Mercury*, before he took off his Finger; which having done, he made his Obſervations. For as the Plant imbib'd the Water, the *Mercury* roſe up in the ſmall Tube, following the Water, and ſhewing by its Height, the Force of imbibing: And for every Inch that the *Mercury* roſe, Water would have riſen 13 $\frac{2}{3}$ Inches, as it is ſo much lighter ſpecifically than Water.

With this Gage our Author made Experiments upon Roots, Branches in an erect, Branches in an inverted Situation, large ones, as well as ſmall, even to Sprigs of Plants and Fruits; and found, that all of them imbib'd the Water, ſo as to raiſe the *Mercury* in the ſmall Tube, but to different Heights. The *Mercury* was raiſed the quickeſt, and to the greateſt height (*viz.* 12 Inches,

Inches in 7 Minutes) by a Nonpareil-Branch two Foot high, with 20 Apples, and ſeveral lateral Branches, whoſe tranſverſe Section was $\frac{5}{8}$ of an Inch diameter. It is to be obſerv'd, that the *Mercury* roſe higheſt in a warm Sun; but the whole force of imbibing was much greater than the *Mercury* could ſhew; becauſe ſeveral Bubbles of Air came out of the Section of the Branch, as the Water went up, which happ'ned moſtly in large Branches, which raiſed the *Mercury* but to a ſmall height: But Branches ſtripp'd of their Leaves, hardly raiſed it all.

He made Peaſe imbibe Water, under the preſſure of great Weights; and found, that Peaſe, when ſwell'd, ſuſtained a Weight equal to 1600 ℔, by their force of imbibing, or growing.

The force of imbibing, he ſhews, to be owing to the attractive power of the Particles of which Plants are made up; all kinds of Wood (even Cork) when their Particles are well ſoak'd, being heavier than Water.

N.B. *It is very difficult to ſoak Cork ſo thoroughly as to make it heavier than Water: But I found it true, in a Tube where a Cork had been ſeal'd up with Water for four Years, ſo as to be moveable in the ſaid Water.*

CHAP.

CHAP. III.

Experiments, ſhewing the Force of the Riſe of the Sap in the Vine, in the Bleeding Seaſon.

MR. *Hales*, by a very curious Contrivance of Glaſs-Tubes, ſcrew'd and cemented at the top of one another, and altogether fixed to the Stem of a Vine, obſerv'd, that in the bleeding Seaſon (*viz.* in *March* and *April*) the Sap will riſe into the Tubes, by the force of the Roots and Stem, to very great heights, even above the Top of the Vine, as it did up to the Top of a Tube 25 Feet high (in two Hours) and ran out at the ſaid Tube, tho' 7 Feet above the Top of the Vine. But the Force was ſuch, that the Mercury, in a Gage (in other Experiments) was puſh'd up 32 $\frac{1}{2}$ Inches high, which was equivalent to above 43 Feet of Water. This is a much greater Force than the Blood is impell'd with in Animals: Which laſt Force he tried in the Crural Artery of an Horſe, of a Dog, and of a Deer: For having tied them down alive upon their Backs, and fixed a Glaſs-Tube to the Crural Artery open'd, he found, that in the Horſe the Blood roſe above 8 Feet, in the Dog about 7 Feet, and in the Deer not quite 6: And theſe Heights are 5, 7, and 8 times leſs than the Height to which the Sap is raiſed by the Vine. The Sap riſes all Night, but moſt in the Morning of a warm Day; but in very hot Weather it ceaſes to riſe about Ten in the Morning, and then begins again to riſe about 3 or 4 in the Afternoon; and during the great Heat, Bubbles of Air drawn in by the Roots, come up the Tube from the Section of the Stem, the Sap in the Tube ſinking and riſing, as the Weather is cloudy or clear, like the Liquor in a Thermometer.

That

That the Force was not only in the Root, but in the Stem and Branches, appear'd by ſeveral Experiments, one of which was, that a Vine, at 44 Feet, 4 Inches in diſtance from the Root, puſh'd up Sap with the Force of a Column of 30 Feet, 11 Inches of Water.

By a nice Experiment he ſhews, that the Sap does not come indifferently thro' all the Interſtices of the Wood; but is confined to its proper Veſſels.

The Fibres, or Capillary Tubes, out of the bleeding Seaſon, cannot protrude the Sap beyond their Orifices in the Section of the Stem; but, aſſiſted by the Perſpiration of the Leaves, the Sap will riſe in that very Branch, which would imbibe Water, if it was cut off, and a Tube applied to it with Water, poured in; nay, it wou'd alſo imbibe Water at the ſmall End.

CHAP. IV.

Experiments, ſhewing the ready lateral Motion of the Sap, and conſequently the lateral Communication of the Sap-Veſſels: The free Paſſage of it from the ſmall Branches towards the Stem, as well as from the Stem to the Branches. With an Account of ſome Experiments relating to the Circulation, or Non-Circulation of the Sap.

THE Analogy that there is in many reſpects between Plants and Animals, has made ſeveral ingenious Men imagine, that the Sap muſt circulate in Plants, as the Blood does in Animals; and, fond of the Hypotheſis, they have contented themſelves with a few Experiments that ſeem to confirm the Notion: And inſtead of making a ſufficient Number of Experiments

riments and Obſervations, and comparing them impartially, they have ſupplied the reſt by Conjectures, and endeavour'd to ſupport their Hypotheſis by other new Suppoſitions. It is upon this account that they have ſuppoſed particular Veſſels (like Arteries) to carry up the Sap from the Roots to the Extremities or Leaves, and others (like Veins) to bring down the Sap back again to the Roots; imagining the former to be chiefly in the middle of the Wood, and the latter to come down between the Bark and the Wood: But they did not conſider, that a Plant is very differently nouriſhed from an Animal, and therefore requires a very different Mechaniſm and Structure for its Growth and Support.

Our Author has diſproved the above-mentioned Opinion, by ſeveral Experiments and Obſervations; the chief of which are the following.

1. Having cut a long Notch in a growing Branch, which had Leaves on, he obſerved, that the Notch was moiſt at the Bottom towards the Root, and not at the Top.

2. He made the ſame Experiment with another Branch ſet in Water, which imbib'd the Liquor at a tranſverſe Section below the Notch, and found the Notch as before.

3. He obſerv'd, that in the Spring the Sap riſes plentifully between the Bark and the Wood, and that chiefly in Vines; and that if a Ringlet of Bark be taken off all round in bleeding Trees, the Bleeding abates moſtly in the upper part of the bare place.

4. The Sap will move any way by the Attraction of the ſame Capillary Tubes, and Perſpiration of the Leaves; ſhewn by an Apple Tree-Branch, which drew up Moiſture plentifully at the ſmall End; by a Tree, which throve, tho' its Root was taken out of the Ground,

Ground, receiving its Nouriſhment only from two other Trees, to which it was inarch'd, and from which it was ſuſpended; as alſo by a Bough with two Branches cut off from a Tree, one Branch of which out of the Water preſerv'd its Verdure, and receiv'd its Nouriſhment from the other Branch immers'd in Water, whilſt another Bough of the ſame ſort without that help, immediately wither'd.

That there is a lateral Motion of the Sap thro' Veſſels, whereby the longitudinal Capillary Fibres communicate with one another, he prov'd chiefly by the following Experiment, *viz.* He cut four long Notches, one above another, (but looking towards the ſeveral Cardinal Points) in a pretty large Branch, whoſe Section ſome Inches below the ſaid Notches, was immers'd in Water; and found, that by the Attraction of the Fibres and Perſpiration of the Leaves, the Moiſture roſe plentifully, notwithſtanding that the Continuity of the greateſt part (if not of all) the longitudinal Fibres was deſtroy'd by thoſe deep Notches.

This lateral Motion may alſo be deduc'd, from obſerving, that Branches will grow out of Roots, and thrive very well; and that Elders, Sallows, Willows, Briars and Vines will grow, by putting either End of a Stem or Branch into the Ground.

He obſerves, that there is indeed, an alternate Receding and progreſſive Motion of the Sap, which recedes when cold Weather and Rain ſucceeds hot Sunſhine, the Trees then imbibing the Rains and Dews which fall upon the Leaves; but upon the return of the hot Weather, the Perſpiration is renew'd, and the Sap riſes again. This is ſufficient to account for Jeſſamine and the Paſſion-Tree receiving a gilded Tincture below the gilded inoculated Bud; which has been given as an Inſtance of the Circulation of the Sap: But the *Ilex*

grafted on the Engliſh Oak, as it keeps (whilſt the Oak ſheds) its Leaves in Winter, ſhews that the Sap does not circulate.

CHAP. V.

Experiments, whereby to prove, that a great Quantity of Air is inſpir'd by Plants.

OUR Sagacious Author makes it appear in this Chapter, that Plants imbibe a great deal of Air, not only that which comes up with the Sap from the Earth (the Earth containing Air both in an elaſtick and in a fix'd ſtate,) but alſo take it in thro' the old Eyes and Bark, eſpecially at Night, when the Plants are in an imbibing State.

The Experiments whereby he proves the ſaid Aſſertion, are chiefly theſe, viz.

1. An open, empty, Glaſs-Tube ſet upright, had an Apple-Branch cemented in its upper end, the other End ſet in a Veſſel of Water; upon which the Air went up into the Branch out of the Tube, as appear'd by the riſing of Water into the Tube.

2. A Cylinder of Birch of about ¾ Inch Diameter, with the Bark on, cemented in the middle, to the Top of an Air-Pump Receiver, ſo as to fill up the Hole at the Top of the ſaid Receiver, had its lower End immers'd in a Veſſel of Water under the Receiver, upon the Plate of the Air-Pump. Then in working the Pump, Air came out at the Bottom of the ſaid Cylinder, bubbling

(bubbling up thro' the Water in the Veſſel) whether erect or inverted, cementedcloſe on the Top-Section, or not, and whether the old Eyes in the Sides were cemented up, or left bare, the Bark it ſelf being pervious to Air; but then it took in leſs Air than when the old Eyes and upper Section were open.

3. When Water (pour'd into a Cylindrick Glaſs fix'd over the Receiver) cover'd that part of the Wood which was out of the Receiver, no Air came thro' the Bottom; and when the Water was taken away from covering the upper part of the Wood, the Air did not come in thro' the Bottom, till the Top of the Cylinder of Wood had its Bark well dried.

4. He obſerves with Dr. *Grew*, that the Mouths of the Air-Veſſels are very viſible in the Leaves of Pines, and in the Trunks of ſeveral Plants, with a Microſcope; and in ſome even without a Glaſs.

5. In making Experiments upon young Shoots, as their Veſſels are full of Sap, he did not find that the Air paſs'd into or thro' them; (the Air in them being probably become fix'd) but it came in freely at the Roots applied to the Air-Pump in the ſame manner as in the Piece of Birch.

CHAP. VI.

A Specimen of an Attempt to analyze the Air, by a great Variety of Chymio-Statical Experiments, which ſhew, in how great a proportion Air is wrought into the Compoſition of Animal, Vegetable, and Mineral Subſtances; and withal how readily it reſumes its former elaſtick State, when in the Diſſolution of thoſe Subſtances it is diſingaged from them.

THE excellent Mr. *Boyle*, among his many Diſcoveries, found, that Air was producible from Vegetables, by putting Grapes, Plums, Gooſe-berries, Cherries, Peaſe, and ſeveral other ſorts of Grain into exhauſted and unexhauſted Receivers, where they continued ſeveral Days, emitting great Quantities of Air. But Mr. *Boyle* did not ſhew the way to know exactly how much Air a certain Quantity of Vegetable Subſtance will produce.

The incomparable Sir *Iſaac Newton* (*Query* 31, of his *Opticks*) obſerves, that "true permanent Air "ariſes by Fermentation or Heat, from thoſe Bo- "dies which the Chymiſts call *fixed*, whoſe Par- "ticles adhere by a ſtrong Attraction, and are not "therefore ſeparated and rarified without Fermentati- "on. Thoſe Particles receding from one another "with the greateſt repulſive force, and being moſt "difficultly brought together, which upon Contact "were moſt ſtrongly united. And (*Query* 30.) Denſe "Bodies by Fermentation rarify into ſeveral ſorts of "Air, and this Air by Fermentation, and ſometimes "without it, returns into denſe Bodies.

Mr.

Mr. *Hales*, from the Conſideration of this, and ſome of his own Experiments, concluded, that there muſt be Air not only in an elaſtick, but in a fix'd State, in Vegetables and other Subſtances, and therefore that the Air-Pump wou'd not be ſufficient to get all the Air out of Bodies: For which reaſon he contriv'd ways of getting the Air out of ſeveral Subſtances by Fire (diſtilling them,) and by Fermentation, and made uſe of a very ingenious, but ſimple and certain Method, of meaſuring the Quantity of Air generated from (or abſorb'd by) the ſeveral kinds of Bodies upon which he made Experiments. The Deſcription of his Hydroſtatical Gages, and the Manner of making his Experiments, I cannot give better, or ſhorter, than in his own Words (*See Page* 157.)

" In order to make an Eſtimate of the Quantity of " Air which aroſe from any Body by Diſtillation or " Fuſion, I firſt put the Matter which I intended to " diſtil, into a ſmall Retort *r* (*Figure* 4.) and then at " *a* cemented faſt to it the Glaſs-Veſſel *a b*, which was " very capacious at *b*, with a Hole in the bottom. I " bound a Bladder over the Cement, which was made " of Tobacco-pipe-Clay and Bean-flower, well mixed " with ſome Hair, tying over all four ſmall Sticks, " which ſerv'd as Splinters to ſtrengthen the Joynt; " ſometimes, inſtead of the Glaſs-Veſſel *a b*, I made " uſe of a large Bolt-head, which had a round Hole " cut, with a red-hot Iron-ring, at the bottom of it; " through which Hole was put one Leg of an inverted " Syphon, which reached up as far as *z*. Matters being " thus prepared, holding the Retort uppermoſt, I " immerſed the Bolt-head into a large Veſſel of Water, " to *a*, the top of the Bolt-head; as the Water ruſhed " in at the bottom of the Bolt-head, the Air was " driven out through the Syphon: When the Bolt-

head

" head was full of Water to z, then I closed the out-
" ward Orifice of the Syphon with the End of my
" Finger, and at the same time drew the other Leg
" of it out of the Bolt-head, by which means the
" Water continued up to z, and could not subside.
" Then I placed under the Bolt-head, while it was in
" the Water, the Vessel *xx*; which done, I lifted the
" Vessel *xx*, with the Bolt-head in it, out of the Water,
" and tyed a waxed-Thread at z to mark the height
" of the Water: And then approached the Retort
" gradually to the Fire, taking care to skreen the
" whole Bolt-head from the Heat of the Fire. The
" Descent of the Water in the Bolt-head shewed the
" Sums of the Expansion of the Air, and of the Matter
" which was distilling: The Expansion of the Air alone,
" when the lower part of the Retort was beginning to
" be red-hot, was at a Medium, nearly equal to the
" Capacity of the Retort, so that it then took up a
" double Space; and in a white and almost melting
" Heat, the Air took up a triple space or something
" more: For which reason the least Retorts are best
" for these Experiments. The Expansion of the distil-
" ling Bodies was sometimes very little, and some-
" times many times greater than that of the Air in
" the Retort, according to their different natures.

" When the Matter was sufficiently distilled, the
" Retort, *&c.* was gradually remov'd from the Fire,
" and when cool enough, was carried into another
" Room, where there was no Fire. When all was
" throughly cold, either the following Day, or some-
" times 3 or 4 Days after, I mark'd the Surface of
" the Water *y*, where it then stood; if the Surface
" of the Water was below z, then the empty Space
" between *y* and z, shewed how much Air was
" generated, or rais'd from a fix'd to an elastick
" State, by the Action of the Fire in Distillation: But

" if

" if *y* the Surface of the Water was above *z*, the ſpace
" between *z* and *y*, which was filled with Water, ſhew-
" ed the Quantity of Air which had been abſorbed in
" the Operation, *i. e.* was changed from a repelling
" elaſtick, to a fixed State, by the ſtrong Attraction
" of other Particles, which I therefore call *abſorbing*.
" When I would meaſure the Quantity of this new
" generated Air, I ſeparated the Bolt-head from the
" Retort; and putting a Cork into the ſmall End of
" the Bolt-head, I inverted it, and poured in Water
" to *z*. Then from another Veſſel (in which I had a
" known Quantity of Water by Weight) I poured
" in Water to *y*; ſo the Quantity of Water which was
" wanting, upon weighing this Veſſel again, was equal
" to the Bulk of the new generated Air. I choſe to
" meaſure the Quantities of Air, and the Matter from
" whence it aroſe, by one common Meaſure of cubick
" Inches, eſtimated from the ſpecifick Gravities of the
" ſeveral Subſtances, that thereby the Proportion of
" one to the other might the more readily be ſeen.
" I made uſe of the following means to meaſure the
" great Quantities of Air, which were either raiſed
" and generated, or abſorbed by the Fermentation
" ariſing from the Mixture of Variety of ſolid and
" fluid Subſtances, whereby I could eaſily eſtimate the
" ſurpriſing Effects of Fementation on the Air, *viz.*
" I put into the Bolt-head *b* (Fig. 5.) the Ingredients,
" and then run the long Neck of the Bolt-head into the
" deep cylindrical Glaſs *a y*, and inclined the in-
" verted Glaſs *a y*, and Bolt-head almoſt horizontally
" in a large Veſſel of Water, that the Water might
" run into the Glaſs *a y*; when it was almoſt up to
" *a*, the top of the Bolt head, I then immerſed the
" Bottom of the Bolt-head, and lower part *y* of the
" cylindrical Glaſs under Water, raiſing at the ſame
time

" time the End *a* uppermoſt. Then before I took
" them out of the Water, I ſet the Bolt-head and
" lower part of the cylindrical Glaſs *a y* into the ear-
" then Veſſel *x x* full of Water, and having lifted all
" out of the great Veſſel of Water, I marked the
" Surface *z* of the Water in the Glaſs *a y*. If the In-
" gredients in the Bolt-head, upon fermenting, gene-
" rated Air, then the Water would fall from *z* to *y*,
" and the empty ſpace *z y*, was equal to the Bulk of
" theQuantity of Air generated: But if the Ingredients
" upon fermentation, did abſorbe, or fix the active
" Particles of Air, then the Surface of the Water
" would aſcend from *z* to *n*, and the Space *z n*, which
" was filled with Water, was equal to the Bulk of Air,
" which was abſorbed by the Ingredients, or by the
" Fume ariſing from them: When the Quantities of
" Air, either generated or abſorbed, were very great,
" then I made uſe of large chymical Receivers, inſtead
" of the Glaſs *a y*: But if theſe Quantities were very
" ſmall, then inſtead of the Bolt-head, I us'd a deep
" cylindrical Glaſs, or a common Beer-Glaſs inverted,
" and placed under it a Vial or Jelly-Glaſs, taking
" care that the Water did not come at the Ingre-
" dients in them: Which was eaſily prevented, by
" drawing the Water up under the inverted Glaſs
" to what heiglit I pleaſed, by means of a *Syphon*.
" I meaſured the Bulk of the Spaces *z y* or *z n*, by
" pouring in a known Quantity of Water, as in
" the foregoing Experiment, and making an allow-
" ance for the Bulk of the Neck of the Bolt-head,
" within the Space *z y*.

By our Author's Experiments made upon ſeveral Subſtances, it appear'd, that ſome by Diſtillation, and ſome by Fermentation, wou'd generate Air, others wou'd abſorb it; and ſome again, wou'd abſorb

Air

Air at one time, and generate it at another; and oftentimes that more Air was generated by Fermentation than by Fire. I have made a Table, to ſhew at one View what the Effects were.

Bodies which generated Air.	*Bodies which abſorb'd Air.*	*Bodies which ſometimes generated, and ſometimes abſorb'd Air.*
Hog's Blood.	Salt of Sal Armoniac.	Gun-Powder.
Tallow.	*Phoſphorus.*	Apples maſh'd.
Fallow-Deer's Horn.	Acid Spirits.	Filings of Steel and *Aqua fortis.*
Oyſter-ſhells.	Lime.	Scurvygraſs-Leaves
A Piece of Oak.	*Pulvis urens.*	*Sal Armoniac* mix'd with Oil of Vitriol.
Indian-Wheat.	Brimſtone-Matches burning.	Spirit of Turpentine mix'd with Oil of Vitriol.
Peaſe.	A burning Candle.	Vineger pour'd on Oyſter-ſhells.
Muſtard-Seed.	Living Animals. *e.g.* a Rat and a Cat.	Limon-Juice.
Amber.	Humane Lungs in breathing.	Ale.
Tobacco.	Filings of Iron with Sp. of Nitre.	Malaga-Raiſins.
Oil of Anniſeed.	Filings of Iron with Spt. of Hartſhhorn.	
Oil of Olives.	Filings of Iron with Spirit of Sal Armoniac.	N. B. *Moſt of the abovemention'd Subſtances were in an abſorbing State in cold Weather.*
Honey.		
New-caſtle-Coal.		
Wax.		
Earth.		
Salt.		
Antimony.		
Pyrites from *Walton*-Heath.		
Sea-Salt.		
Nitre.	N. B. *When a lighted Candle is put into that Air which had been foul'd (by a Candle burning and going out in it) tho' it burn'd but a fifth part of the Time that it had done before, it abſorb'd as much Air; which ſhews that Air loaded with Vapours is more apt to loſe its Elaſticity, than clear Air.*	
Calculus humanus.		
Vitriol.		
Bones.		
Red Lead.		
Chalk.		
Wheat and Barley.		
Filings of ♂ with Oil of Vitriol and Water.		

As ſeveral Perſons may be willing to try a great many of Mr *Hale's* Experiments, I thought proper to mention here, that the Experiments on Subſtances that generate Air, made with the Glaſs-Retort, are difficult and tedious; but a Musket-Barrel with the Touch-hole welded up, and then turn'd into a Semi-circle, makes a very commodious Iron-Retort, whoſe cloſe End being put into the Fire of a Smith's Forge, will be ſo heated with a little blowing, as to get the Air out of any Subſtance in a very little time; having firſt twiſted on upon the open End of the Barrel one End of a Leaden Syphon (which will be very tight, with only a little Sheep's Leather between) whilſt the other End of the Syphon goes up a little way into an inverted Chymical Receiver fill'd with Water. As the Air gets out of the Subſtances diſtill'd, it comes bubbling up thro' the Water, which it depreſſes by its Bulk. N. B. *Antimony and Sulphur muſt not be made uſe of in this Barrel; becauſe they will demetallize the Iron; nor Lead, becauſe it will ſweat out thro' the Iron.*

It appear'd by ſeveral Experiments, that the Air which was got out of the Subſtances above-mention'd, was true permanent Air; for it had, and, after many days, continu'd to have, both the Weight and Elaſticity of common Air, of which a cubick Inch weighs $\frac{2}{7}$ of a Grain, and after it had been compreſs'd, it wou'd expand again to its former Dimenſions.

Moſt of that Air had a poiſonous Quality, a Sparrow dying as it was put into the Air made of Oak, 11 days after the ſaid Air had been made. So Mr. *Boyle* found the Air drawn by the Air-Pump from Bread and ſome other Subſtances, to have a poiſonous Quality.

Air generated by *Indian* wheat and by Peaſe, and moſt other Subſtances, flaſh'd when the Candle was applied to it.

The

The moſt ſolid Parts of Animals or Vegetables commonly generated moſt Air: Of Vegetables, Oak and Peaſe generated the moſt Air; Oak $\frac{1}{3}$ of its Weight, and 216 times its Bulk, and the Peaſe more than $\frac{1}{3}$ of their Weight and 396 times their Bulk. But of all Subſtances, a Stone taken from the Bladder of a Man generated moſt Air, namely a Quantity of Air 645 times the Bulk, and above half the Weight of the Stone.

Our indefatigable Philoſopher, to judge the better of the reſult of theſe Experiments, contrived a Gage with a Tube ſeal'd at one end, and having the other (open End) immers'd in Mercury cover'd with Honey, which in the riſe of the Mercury in the Glaſs wou'd leave a mark to ſhew how far the Mercury had riſen, (See the Deſcription and Draught of it in his Book, *Page* 205;) by the force of compreſs'd Air, or of the Air that is generated from ſeveral Subſtances whilſt confin'd; and propoſes this Gage for meaſuring the Depth of the Sea, which muſt anſwer better than any Contrivance hitherto tried.

From obſerving that it wou'd require a Force 48 times greater than that of the Atmoſphere, to compreſs the Air that comes out of an Apple into the ſame ſpace again (which in an Apple of 16 ſquare Inches Surface, is equal to 11776 Pounds,) he concludes that the greateſt part of the ſaid Air muſt be in a fix'd State, and preſerv'd in that State by the Attraction of the Particles of Air and Apple in their Coheſion; otherwiſe Fruits and all other Subſtances that contain much Air, wou'd be torn to pieces by its repellent Force, with a greater Exploſion than that of Gunpowder; and therefore that the Air when extricated, does not conſiſt of fibrous Particles wound up like little Springs, but of Particles which do not touch

one another, but have a repellent Force, which is reciprocally proportionable to the Diſtances of the Particles: Agreeable to what Sir *Iſaac Newton* ſays in his Opticks, *Quer.* 31. " The Particles, when they " are ſhaken off from Bodies by Heat or Fermen- " tation, ſo ſoon as they are beyond the reach of " the Attraction of the Body, receding from it, " as alſo from one another, with great Strength, " and keeping at diſtance, ſo as ſometimes to take " up above a million of times more ſpace than they " did before in the form of a denſe Body; which " vaſt Contraction and Expanſion ſeems unintel- " ligible, by feigning the Particles of Air to be " ſpringy and ramous, or rolled up like Hoops, or " by any other means than by a repulſive Power.

Our Author made ſome Experiments upon a Calve's Lung's, which help to confirm Dr. *James Keil's* Aſſertion, *viz.* that the inner Surface of a Man's Lungs is equal to about 150 * ſquare Feet, which is about 10 times more than the Surface of the whole Body. Then by ſeveral Experiments and curious Obſervations, found, that of the Air which is inſpir'd by the Lungs in an Hour (*viz.* 48000 cubick Inches) about 353 Inches, or 100 Grains in Weight are deſtroy'd in that Time.

He ſhews, that the reaſon that Sulphureons Air is pernicious, is becauſe Sulphureous Air loſes its Elaſticity ſo faſt, that the Lungs will ſubſide in it, and the Blood conſequently ſtagnate. He concludes, that when the Particles of ſuch Air are united into pretty large *Moleculæ* in a fix'd State, they are too big to enter the Veſicles of the Lungs.

---

* *Since my making this Abſtract, Mr.* Hales *told me, that the inner Surface of the Lungs is double the Quantity here expreſs'd, which was ſo ſet down in his Book by miſtake.*

He

He ſhews the Miſtake of thoſe that ſuppos'd the Air did not loſe its Elaſticity, but its *Vivifying Spirit* by Vapours, by an Experiment made upon a Dog, into whoſe Lungs he drove by Force that effœte Air which the Dog by his own Action cou'd not draw into his Lungs; and whereas the Dog, then, was juſt expiring by the ſubſiding of his Lungs, he kept him alive by the forcible Impulſion of that very Air, as long as he cou'd blow up the Dog's Lungs with it.

He makes it appear, that a cloſe warm Air without a Communication with the outward Air, to carry off the Vapours, muſt be unfit for breathing long; and therefore condemns the uſe of *German* Stoves.

That when Animals are kill'd by Lightning without any viſible Wound, it is becauſe the Sulphureous, Steams have deſtroy'd the Elaſticity of the Air about them; as appears by diſſecting ſuch Animals, whoſe Lungs are always found compreſs'd together without any Air in them. The ſame alſo appears in Men that have been kill'd by Damps in Mines.

Then he gives the Deſcription of an Inſtrument whereby 4 or 5 Quarts of the ſame Air, (which in the common way can ſerve one Man to breathe very little above a Minute) may ſerve 8 ½ Minutes to breathe, by drawing in the Air which has been in the Lungs thro' 4 Diaphragms of Flannel dipp'd in a *Lixivium* of highly calcin'd Salt of Tartar, and afterwards dried, and blowing back the ſame Air in ſuch manner that it may at every Inſpiration return thro' the ſaid Diaphragms; which is done by means of two Valves. The Moiſture and Sulphureous Vapours of the Air are by this Contrivance ſtopp'd in paſſing thro' the Flannel, and thereby the Air continues wholeſome much longer than it

wou'd

wou'd in the common way of breathing: This he propoſes as uſeful to thoſe who have occaſion to go into Places where the Air is infectious; but where the noxious Vapours are not very denſe, Mufflers only of Cloth or Flannel impregrated with Salt of Tartar may ſerve, as he found by ſeveral Experiments.

He ſhews afterwards the Reaſons why *Pulvis fulminans* has more Force than Gun-Powder, and confirms Sir *Iſaac Newton*'s account of Fire and Flame (*Query* 9, and 10, of his *Opticks*;) but ſhews Dr. *Nieuwentyt*'s, Dr. *Boerhaave*'s and Monſ *L'Emery*'s Account of Fire to be erroneous; but that it was not the matter of Fire, but Elaſtick Air which that laſt nam'd Gentleman had ſo often obſerv'd to be loſt in the Analyſis of Bodies.

That the artifical Sulphur made by Monſ. *Geofroy*, was inflammable chiefly on account of the Air ſupplied to it by the Oil of Tartar.

That if Fire was a peculiar Matter in Sulphur, it ought to dilate, not to condenſe, the Air; as it appears to do by Experiment. That Fire conſiſts in the Action and Reaction between repellent Air and attracting Sulphur; and that Heat is communicated by the Æthereal Medium mention'd by Sir *Iſaac Newton* in the laſt Edition of his *Opticks*.

That Fermentation will diſſolve Bodies without the Help of included Fire, acting according to its ſeveral Degrees; a lower Degree of it (ſuch as attends every inteſtine Motion) not being inconſiſtent with the healthy State of Plants and Animals.

That acid Particles by their great attractive Force diſſolve Bodies, ruſhing towards their Particles, ſo as to excite Heat, and ſhake aſunder ſome of them, and turn them into Air.

That

That Air, in a fix'd State, is the Bond of Union, which makes Salts durable, and which keeps together the Solid Parts of Vegetables, of Animals, and even Globules of the Blood.

That the Air in a moiſt State is ſooner abſorb'd by Sulphureous Steams, than in a dry State; for a Candle which in a dry Receiver burn'd 70″, burn'd but 64″ in the ſame Receiver when it was fill'd with the Fumes of hot Water; but yet abſorb'd one fifth part more of Air. But the abſorbing Subſtances loſe their Force of acting, when united in a large Body, Brimſtone in a Roll abſorbing no Air, tho' it does it ſo plentifully when reduc'd into Minute Particles.

That ſome of the Food of Animals generates, and ſome abſorbs Air, and the Digeſtion in a healthy State is beſt perform'd when there is but a little Air more generated than is abſorb'd.

From the Conſideration of the ſeveral Experiments made by our Author, in his Analyſis of Air, he makes it appear that our Atmoſphere is a Chaos of different Particles, ſome of which are elaſtick, and ſome unelaſtick; and that the elaſtick Parts are endued with very different Degrees of Elaſticity, according as they are bigger or leſs, more or leſs ſolid, more or leſs watry; and therefore that ſome are eaſily and ſome more difficultly reducible to a fix'd State.

That it is chiefly by the Change of Air from a fix'd to an elaſtick, and from an elaſtick to a fix'd State, that this beautiful *Frame of Things* is maintain'd in a continual round of the Production and Diſſolution of animal and vegetable Bodies; and therefore, that there is as much reaſon to adopt Air among the Chymical Principles, as acid Sulphur; tho' it has hitherto been rejected by the Chymiſts.

This will be continu'd in the next.

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TO HIS

ROYAL HIGHNESS *FREDERICK* Prince of *WALES*.

SIR,

SINCE your ROYAL HIGHNESS has graciouſly condeſcended to inſcribe Your Name in the Book of Charters and Laws of the *Royal Society*, as a Token of your Favour and Encouragement of it, I preſume to offer this Thirty-fifth Volume of *Philoſophical Tranſactions*, ſome of its lateſt Productions, to your ROYAL HIGHNESS's more immediate Protection.

This alone would be ſufficient Honour to the ſeveral Authors of the Tracts contained in it: But they muſt ſtill have a private Satisfaction in knowing, that they are

a ſub-

ſubmitted to the Obſervation of a Prince, whoſe Diſcernment renders him a compleat Judge of the Merits of their Labours; and whoſe Benevolence, at the ſame Time, will readily incline Him to think favourably of them.

If, beſides this, they ſhould chance to furniſh any Matter for your learned, or ingenious Entertainment, I beg your ROYAL HIGHNESS to receive it, as one Inſtance of the grateful Senſe conceived of the late Honour done us, and particularly, of the profound Reſpect, with which I am,

Your ROYAL HIGHNESS's

Moſt Devoted, Obedient,

and Humble Servant,

William Rutty, *R. S. Secr.*

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, and Labours

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. XXXV. For *July*, *August*, *September*, *October*, *November* and *December* 1727, and for the Year 1728.

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M. DCC. XXIX.

Jos: Banks

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PHILOSOPHICAL TRANSACTIONS.

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201 A Butilon, *Dod.* 656.
202 — Americanum, fructu ſubrotundo pendulo, è capſulis veſicariis criſpis conflato.
203 — Alkekengi Barbadenſe, patulum parvo flore; fructu amplo mucrone productiori.
204 — Indicum majus, *T. Inſt.* 151.
205 — Curaſſavicum; foliis Origani incanis; flore viete ſulphureo, fundo purpureo, *Boerh. Ind. Alt.* II p. 66.
206 — Verticillato ſimile; foliis anguſtioribus, utrinque acuminatis.
207 — Virginianum perenne majus; flore luteo amplo; fructu minimo.
208 — Ammi perenne Moris, *Umb.* 22.
209 Apocynum ſcandens, Africanum; Convolvuli minoris folio & caule hirſutis, *Pluk. Phyt.* Tab. 137. f. 4.
210 Bidens Americana Apii folio, T. 462.
211 — Americana nodiflora foliis latis acuminatis. Chryſanthemum Conyzoides, nodiflorum; ſemine roſtrato bidente, *Sloan.* Hiſt. Vol. I. p. 62. Tab. 154. f. 4.

212 — Caroliniana ; florum radiis latiſſimis, inſigniter dentatis ; ſemine alato, per maturitatem convoluto.

213 — Bugloſſum Luſitanicum, bullatis foliis Moris, *Hort. Blæſ.*

214 — Caltha Africana, flore intus albo foris Violaceo, *T Inſt.* 499.

215 Cardamine impatiens, vulgo Sium minus impatiens, *Gerard.*

216 — Impatiens altera hirſutior, *Raii Synopſ.*

217 — Clematitis peregrina foliis perinciſis, *C. B.* 300.

218 — Corchorus ſive Melochia, *J.B.* 2. 982.

219 — Coma aurea Africana fruticans, foliis Crithmi marini, *H. Amſ.* 2. 99.

220 —Kali ſpinoſum, foliis longioribus & anguſtioribus, *T. Inſt.* 247.

221 — — Foliis craſſioribus & brevioribus, *Ibid.*

222 — Lactuca Canadenſis, altiſſima, latifofolia ; flore leucophæo, *T. Inſt.* 474.

223 Lathyrus ſylveſtris major flore minore ; foliis longius mucronatis.

224 Lavandula folio diſſecto, *C.B.* 216.

225 — Lychnis Hiſpanica major latifolia flore albo.

226 — Maritima repens, C. B. 205.

227 — — Anglica, amplioribus foliis.

228 — Orientalis Bupleuri folio, *T. Cor.* 24.

229 — — Calyce pyramidato, ſtriato, longiſſimo ; *T. Cor. ib.*

230 — — Foliis carnoſis ; caule ramoſiſſimo ; floſculis minutis onuſto.

Lychnis Orient. Saponariæ folio & facie, flore parvo & multiplici.

231 — Fruteſcens, Myrtifolia, Been albo ſimilis, C.B. 205.
232 — Segetum Meridionalium annua, hirſuta; floribus rubris, uno verſu diſpoſitis, *Boerh. Ind. Alt.* 214.
233 — Sylveſtris viſcoſa rubra anguſtifolia, C. B. 205.
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235 — Umbellifera, montana, Helvetica, *Zanon.* 128.
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244 — Moldavica Betonicæ folio flore cæruleo, *T. Inſt.* 184.
245 — Orientalis, Betonicæ folio flore magno Violaceo, *T. Cor.* 11.
246 — Myrrhis tuberoſa, nodoſa, coniophyllon Moris, *Umb.* 67.
247 Myrrhis folio Podagrariæ Rivin.
248 — Niruri Barbadenſe, folio ovali ſubtus glauco, pediculis florum breviſſimis.

249 Senecio Virginianus, arboreſcens, Atriplicis folio, *Par. Bat.* 225.

250 Sherardia Teucrii folio flore purpureo, *Vaillant Diſc.* p. 49.

Verbena ſolio ſubrotundo, ſerrato; flore Cæruleo, *Sloan. Hiſt.* Vol. 1. p. 171. Tab. 107. f. 1.

II. *Some farther Remarks on* P. Souciet's *Diſſertations againſt Sir* Iſaac Newton's *Chronology, by* Edmund Halley, *L.L.D.* Aſtron. Reg. *In a Letter to Dr.* Jurin, *Coll. Med. & S.R. Soc.*

SIR,

WHEN I gave you my Paper of Remarks on *P. Souciet*'s Diſſertations againſt Sir *Iſaac Newton*'s Chronology, I was oblig'd to take what he was pleas'd to give us out of *Hipparchus*'s Comment on *Aratus*, not having then that Author himſelf by me. Since then, by the favour of a very good Friend, having gotten the *Florence* Edition of *Hipparchus*, *Anno* 1567, I find an Argument very much *ad Hominem*, which the R. P. muſt confeſs will bring the *Argonautick* Expedition full as low as Sir *Iſaac Newton* makes it.

P. Souciet in his fifth Diſſertation, Page 119, 120, finds out a Star of the firſt Magnitude, cloſe adjoyning to that we now call *the firſt Star of Aries*, as it is in the Catalogue of *Ptolemy*, where it is ſaid to be in the *Horn of Aries*, and not in the *Ear*. This Star the R. P. ſuppoſes long ſince to have diſappeared; but that being of old very conſiderable, it was from this firſt Star of *Aries*, the Zodiack began, tho'

tho' for Argument-ſake he is contented to let it begin as Sir *Iſaac* does, with the aforeſaid Star in the Ear or Horn; which *Hipparchus*, in the laſt and 54*th* Page, tells us, in his time follow'd the Equinoctial Colure the 20*th* part of an Hour: And ſuppoſing the Star that has diſappeared, to have been at that time preciſely on the Colure, it muſt differ but 45 Minutes of Right Aſcenſion therefrom: But how he comes to make the difference of Longitude 40 Minutes no way appears, and is *gratis dictum*.

In Page 49 of the ſaid *Florence*-Edition, *Hipparchus*, treating of the riſing and ſetting of the Conſtellations, tells us, that that of *Aries* began to riſe with 18 ½ Degrees of *Piſces* in the Ecliptic, and was wholly riſen with the 24*th* Degree of *Aries*, whilſt the Zodiack paſt the Meridian from 23 ½ Degrees of *Sagittary* to 14 Degrees of *Capricorn*: And again (Page 52) he ſays, that the Conſtellation of *Aries* began to ſet with the 29*th* Degree of *Piſces*, and was wholly ſet with the 26*th* Degree of *Aries*, whilſt the Zodiack paſt the Meridian from 29 Degrees of *Gemini* to 29 Degrees of *Cancer*. He tells us alſo that it was the very ſame Star that both roſe and ſet firſt in that Conſtellation, calling it ὁ ἐπὶ τῷ ἐμπροσθίῳ ποδός, Page 49: And again, Page 52 'tis named ὁ ἐν τοῖς ἐμπροσθίοις ποσίν, or *That in the Fore-feet* of the Ram.

This certainly is the Star P. *Souciet* would place on the Equinoctial Colure, and makes it long ſince to have diſappeared; without enquiring whether the aforeſaid *Data* were not abundantly ſufficient to determine its place in the Zodiack at that time; and without regard to the odd uncouth Poſture he muſt ſuppoſe the Conſtellation of *Aries* to be in, when he makes one or both of the Fore-feet ſo near to, and above the Horn or Ear.

Hip-

Hipparchus expreſsly ſays, that it roſe when 23 $\frac{1}{2}$ Degrees of *Sagittary* was on the Meridian, and ſet when 29 Degrees of *Gemini* paſt it; and taking the middle between thoſe Points, it is plain, that it culminated with about 26 Degrees of *Piſces*, and that it had North Declination; the Exceſs above 180 Degrees ſhewing that the Aſcenſional Difference was about 2 $\frac{3}{4}$ Degrees. But to give the Argument its full Scope, the Right Aſcenſion of 23 $\frac{1}{2}$ of *Sagittary* (allowing *Hipparchus* his Obliquity 23° 51′ 20″) will be found 262° 54′. And that of 29 Degrees of *Gemini* will be 88° 54′: So that this Star was above the Horizon (in the Latitude of 36 Degrees North; to which *Hipparchus* has adapted his Calculation) 12 Hours 24 Minutes or 186 Degrees; whence the Right Aſcenſion of the Star is juſtly concluded 355 Degrees 54 Minutes; and its Aſcenſional Difference preciſely 3 Degrees; which in that Latitude makes its Declination 4 Degrees 7 Minutes North. We have therefore gotten both the Right Aſcenſion and Declination of this ſuppoſed firſt Star of *Aries*.

Let us now ſee what Longitude and Latitude reſults from the aforeſaid Right Aſcenſion with 4 Degrees 7 Minutes North Declination, aſſuming the Obliquity with *Hipparchus*, to have been 23° 51′ 20″; and we ſhall by a juſt Computation, find the Star at that time to have been in 27° 53′ of *Piſces*, with 5° 24′ North Latitude, which therefore was reckoned the place of the Star at that time by *Hipparchus*. Add 2° 40′, for 265 Years between *Hipparchus* and *Ptolemy*, and we ſhall have its place, in *Ptolemy*'s Account, *Aries* 0° 33′, with 5° 24′ North Latitude. But the 22*d* Star of *Piſces* in *Ptolemy*'s Catalogue has the ſame Longitude and Latitude, with ſufficient exact.

actneſs, *viz. Aries* 0° 40′, with North Latitude 5° 20′, and is *Media trium in Lino Boreo Piſcium* (*n Bayero.*) Hence it cannot be doubted but that this Star which *P. Souciet* takes to have been once a Star of the firſt Magnitude, was no other than the ſaid 22*th* of *Piſces*, which in the *Britiſh* Catalogue, fitted to the Year 1690, is put down in *Aries* 22° 29′ $\frac{1}{3}$ with North Latitude 5° 21′.

How *Hipparchus* came to reckon this Star to be in the Fore-foot of *Aries*, does not at preſent appear; but it is not unlikely that theſe Commentaries of his upon *Aratus* were written ſome time before he ſet about making his Catalogue of the fixt Stars; when he might change his Opinion, and replace it in the *Line* of the *Fiſhes*, to which it ſeems more properly to belong.

Be that as it will, we will for once, ſuppoſe with *P. Souciet*, this Star to have been in the beginning of the Zodiack, or of the Conſtellation of *Aries*, and that at the time of the firſt fixing the Colures, that of the Vernal Equinox paſt 15 Degrees in Conſequence thereof. Now *anno ineunte* 1690, this Star being in *Aries* 22° 29′ $\frac{1}{3}$, if we add thereto 15 Degrees, we ſhall have *Taurus* 7° 29′ $\frac{1}{3}$ for the Point in the Ecliptic that was then the beginning of the Zodiack. Now 37 $\frac{1}{2}$ Degrees, at 50 Seconds *per Annum*, gives 2700 Years; from which deducting 1690, we ſhall have 1010 Years before *Chriſt.* But this Star having 5° 21′ North Latitude, the Colure, when it paſt over it, interſected the Ecliptick in 2° 20′ leſs Longitude, which gives the time 168 Years later, or but 842 Years before *Chriſt.* So that *malgrè cette* grande *decouvèrte*, the new Syſtem of Chronology is ſo far from being refuted, that it ſeems to be very much con-

confirmed thereby, at leaſt in the Opinion of the R. *Pere*.

I have aſſumed the Latitude to which *Hipparchus* might have adapted his Calculations, to be 36 Degrees; becauſe I find in Page 14. of the aforeſaid Edition that he makes the longeſt Tropical Day 14 h. 30′: And in Page 29, he tells us, that the Southern Star in the left Foot of *Bootes* (*υ Bayero*) having 27° 20′ North Declination, was above the Horizon, 14 h. 57′; whence it follows, that the Latitude muſt be 36° 5′. He alſo tells us, in the ſame Page 29, that this Star ſet when 22° of *Capricorn* culminated, and 6° of *Taurus* aſcended; repeating the ſame thing in Pag. 39, which leaves no room to ſuſpect that thoſe Numbers are not the ſame that *Hipparchus* had computed. I therefore thought it worth while to enquire in what Latitude 6 Degrees of *Taurus* riſes when 22 Degrees of *Capricorn* is on the Meridian; and with the Obliquity of the Ecliptick, as now we have it, the Latitude reſulting is 35° ½ North; but with the Obliquity allowed by *Hipparchus*, it will be found leſs than 35°.

This I ſay, only to obviate any Objection that may be made by *P. Souciet* to the aforegoing Argument; tho' if he pleaſe to examine it, he will find that an Error of a Degree in the aſſumed Latitude, will by no means invalidate the Proof here given that this *Firſt Star of Aries* could be no other than the middle Star *in Lino Boreo Piſcium*, marked *n* by *Bayer*.

III. *An*

III. *An Account of the* Lumen Boreale, *as ſeen at ſeveral Times. By the Rev. Dr.* Langwith, *Rector of* Petworth *in* Suſſex. *Communicated in a Letter to the* Same.

SIR,

I AM very glad to find that the Accounts of the laſt *Meteor* are of ſo near an Agreement; for I perceive there is little difference but what may be owing to the time and place of Obſervation. We have had the ſame ſort of Lights here ſeveral times ſince my laſt; particularly,

Jan. 4. A Luminous Arch which extended it ſelf from N. E. to Weſt. The Streams all mov'd Weſtward. Wind N. W. *Merc.* 29 ½.

Jan. 5. We had ſomething of the ſame Nature, but hardly enough for Obſervation; and yet, this very Night the Appearances were more remarkable in ſome parts of the Kingdom than thoſe of *October* the eighth. This I was inform'd of by a Perſon of Quality in *Lancaſhire*, who was pleas'd alſo to ſend me the enclos'd Deſcription and Draught, communicated to him by a curious Obſerver at *Liverpool*.

March 2*d*. Between 7 and 8, there was an Arch upon a black Baſis as before, extending it ſelf from N. E. to W. It's height variable, Pyramidal Streams of Greeniſh Light moving Weſtward. About a quarter paſt 8, there ſhot up from the Weſt a Stream of pale Flame-colour about 6 or 7 Degrees broad: It paſs'd over the *Pleiades*, and croſſing the *Meridian* a-

bout 19 Degrees to the North of our *Zenith*, deſcended as low as the Tail of *Urſa Major* which it left a little to the South. It continu'd thus for ſome Minutes and then gradually vaniſh'd. Wind North, *Merc.* about 30.

March 3*d.* The Appearances this Night were ſo extraordinary, that they wou'd require a long deſcription: But I ſhall chiefly take Notice of ſuch particulars as differ'd from thoſe of *October* 8.

1° That inſtead of one *Luminous Arch* in the North, here were *two* and ſometimes *three* one above another. They were diſtinct enough from each other in their upper parts, but blended together towards the *Horizon*, which they *generally* cut about *N. E.* and *N. W.* but ſometimes varied conſiderably from theſe Points.

The ſame Obſervation may be apply'd to the *heights*, for they were alſo variable; and in particular, the inner edge of the lowermoſt Arch was at ſometimes about 6 Degrees above the *Horizon*, at others, conſiderably more or leſs.

I ſuppoſe this extroardinary Appearance was owing to ſeveral diſtinct Collections of Luminous Vapours, which were either at different heights from the Earth, or different diſtances from the Eye.

2*dly*, Several of the more permanent Streams were bent, at times, into irregular Arches of different Curvatures and Poſitions.

Some of them held pretty near the ſame ſhape till they vaniſh'd, others went off moſt commonly in *Tangents* to ſome part of the former Curves.

3*dly*, The Flaſhing Streams from the *Eaſt* ſometimes met with thoſe from the *Weſt*, and ſo form'd continu'd Arches, of a pale colour, which quickly broke and vaniſh'd. No colouring follow'd upon the mixture of theſe Streams.

4*thly*,

4*thly*, The Streams of this kind mov'd moſtly *Southward*, but not to any certain Point; for they were inclin'd to the *Horizon* at all Degrees between 5, or leſs, and 90. There was ſometimes ſuch a ſtrange irregularity in their Motions as can hardly be deſcrib'd; for the Places from whence the Flaſhings were directed ſeem'd to vary every Moment.

As to the more ſteady *Pyramidal* Streams, they generally mov'd *Weſtward*; and tho' ſome of them, at times, ſeem'd to ſtand ſtill, or even move backward; yet I am apt to believe, this irregularity was only Apparent.

5*thly*, A little after the beginning of this *Meteor* there was a faint ruddineſs in the Sky towards *N. E.* and *N. W.*; but when it was in its greateſt perfection, towards 12 o' Clock, I ſaw none of the *Priſmatick* Colours, tho' the Air was then full as light as I have ever known it upon theſe Occaſions. This helps to confirm me in the Opinion that the *Priſmatick* Colours in theſe Meteors were owing to the Sun.

Theſe Appearances began early in the Evening, and held, as I am inform'd, for a good part of the Night. Wind *N. Weſterly*, Merc. above 30.

I am told that theſe Meteors are much more common in the *North* of *England* than here, and that they go by the Name of *Streamers*, *Merry Dancers* or *Petty Dancers*. They alſo pretend to foretel the Weather by them, and ſay, that when the Streamers are green, they betoken Wet Stormy Weather; but when they are yellow, it will be Clear and Dry.

P. S. I don't know whether it is worth while to acquaint you, that *Ariſtotle* has given an *imperfect* Account of *ſome* of theſe *Meteors*.

I am, &c.

Petworth,
March 6, 1726-7.

B. Langwith.

IV. *The Deſcription of an* Aurora Borealis *mention'd in the foregoing Letter.*

My LORD,

ABout Seven 'o Clock at Night I was told that the Meteor call'd by our Sailors, *Merry Dancers*, was viſible, and very bright. Having ſeen ſeveral before, but had no opportunity of being particular in my Obſervation, I went out into the open Air, clear of Houſes, that I might have a better view all round the *Horizon*; from whoſe *Northern* part aroſe ſeveral Streams of Light, as if from behind a black Cloud. They were very many, and I believe, there was no poſſibility of numbering them, their Motion being ſo quick, ſhooting upwards to the *Zenith* with a Motion not be follow'd by the Eye. They had alſo another Motion which ſeemed to be ſide-ways, their higher Ends terminating ſometimes in a ſharp Point, ſometimes in two or three Points; they appeared from the *North-Weſt* to *North-Eaſt*; but were brighteſt in the *North*. Their Colour was pale like that of *Jupiter* through a Teleſcope, but not near ſo bright. Moſt of 'em reach'd the *Zenith*, where mixing with one another, they whisk'd round and form'd an Appearance like the *curling* Flame of a Glaſs-Houſe-Fire; they had a very irregular Motion, ſometimes turning inwards, ſometimes outwards, like the *Pendulum-Spring* of a Watch. This *circular* Light was the brighteſt, and ſeem'd to occupy near ten Degrees of the higheſt part of the Hemiſphere: Several Strokes of Light ſeem'd to dart from it to the *South*; but

but died before they got any conſiderable diſtance. *In the West*, I ſaw two ſmall long Clouds, which interpoſed betwixt me and the light Streams; which I ſaw above the Clouds, and betwixt them, which convinc'd me that this Light (whatever it be) is far above them. I have drawn a Scheme of the whole Horizon, as it appear'd to me. *(See the Figure.)* That bright Star is *Jupiter*, whoſe Place then was 17° in *Aries*, and was about South-Weſt, I gueſs about 20° high. Some of the brighteſt Stars in *Taurus*, *Orion*, and *Aries* appear'd South and South Eaſt; but I have not placed thoſe but by gueſs. In this ſtate I left it: But was told by one that ſaw it after Ten o' Clock, that the whirling Light in the Zenith appeared of ſeveral Colours, as, blue, green, yellow, and reddiſh. But that I did not ſee.

I am, &c.

V. *A Letter from the Reverend Mr.* Edmund Barrel, *Rector of* Sutton *near* Dartford *in* Kent, *to Sir* Hans Sloane, *Baronet, concerning a Shock of an Earthquake felt in that Neighbourhood*, &c.

SIR, *Sutton, Aug. 11th, 1727.*

I had an Information brought to me Yeſterday, on purpoſe to be communicated to your Society; 'twas, that the Earthquake was felt very ſenſibly at a Farm on a Hill called *Skeat-Hill*, which is at the Weſt End of *Lullingſtone*-Park, belonging to *Percival Hart*, Eſq;

Eſq; about 8 Miles South-Weſt from *Dartford*: And the ſame Morning a Piece of Ground, in a Meadow in *Farningham*, about five Miles South of *Dartford*, fell in, ſo as leave a Pit about 8 or 10 Feet over and near as deep; and being on the ſame level with the River, it was (when ſeen that Morning) fill'd with Water, within 3 or 4 feet of the Top; tho' that Spot of Ground, was ſuppoſed to have been as ſound as any about it, Carts having many times gone over that very place. I thought it not improper to trouble you with this, ſuppoſing, that the Curious, will be pleas'd to hear how far the Shock of the Earthquake was felt.

P. S. In my Letter about *Miſſelto*, I mentioned a viſible Proof, that there always was a *germinating* Principle in the Seeds of this Plant, whenever the Berries remained on their Boughs till *May*. The Birds do not often permit any ſuch to be found; but as I have formerly obſerved this, ſo I have met with ſome this Year, whereof I have herewith ſent you a *Specimen*; wherein you will find both old ripe Berries and young green Ones, on the ſame Stalks; and in the old you may ſee (even through their Skin) the little *Germen* putting forth its Head from the Seed or Kernel. I have likewiſe formerly had a Suſpicion that the Plants of *Miſſelto* are ſome *Male*, ſome *Female*: I am now further perſwaded, that it may be ſo; and if this be confirmed by my future enquiries I will make bold to give you an Account of it.

I am, &c.

VI. *A Species of* ſubterraneous Fire, *obſerv'd in the* ſame County; *communicated in a Letter to* Dr. Jurin, *by* Robert Nesbitt, M.D. *Coll. Med. Lond. Lic.*

SIR,

I Here ſend, as you deſired, the beſt Account I can of a Species of *ſubterraneous Fire*, which was firſt taken notice of on the ſecond of *Auguſt* laſt, in a *Marſhy-Field* ſituated in the Pariſh of *Flinx-Hill*, about ten or twelve Miles South Weſt of *Canterbury* in the County of *Kent*.

This Fire began on the ſide of a little Brook near the Water, and continu'd to burn along its Bank, without ſpreading much for ſome Days. Afterwards it appeared on the other ſide, and extended it ſelf the ſpace of ſome Acres over the Field, conſuming all the Earth, where it burnt, into red Aſhes quite down to the Springs; which in moſt places lay four Feet or more deep. On the twenty fourth of *September* I went to ſee it, and found it had conſumed, as well as I cou'd gueſs by my Eye, about three Acres of Ground.

It then burnt in many Places, and ſent forth a great Smoak and ſtrong Smell, very like to that of a Brick-Kiln. It never flamed but when the Earth was turn'd and ſtirred. For ſome ſpace round where it was burning, the Ground felt hot, tho' the Graſs ſeemed no more parch'd than might reaſonably be expected from the Dryneſs and Heat of the Seaſon. I cauſed it to be turn'd up in ſeveral Places, and found the Earth hot

and

and wet near four Feet deep, and much hotter about two feet than near the Surface.

When this Earth was expoſed to the Air, tho' it was very moiſt and not hotter than you might eaſily bear with your Hand, the Heat increas'd ſo faſt, that in a few Minutes it was all over on Fire, like *Phoſphorus* made with *Allom* and *Flower.*

The Soil of the Field is of the ſame nature with that they make the *Turf* of in *Holland*; the Surface of it is always wet, except in extream dry Seaſons. This Year it was ſomewhat more parched and harder than uſual.

I believe, from what has been related, it is not more difficult to account for this Fire, than for thoſe Fires which often happen in *Hay-ricks* when Hay is ſtacked before it is thoroughly made.

P. S. I ſend you a Piece of the Earth, and ſome of the Aſhes. I ſhould have mentioned, that I took ſeveral Pieces of the Earth, before I was able to bring one away, becauſe of its firing; one Piece, in particular, I gave to my Servant, which firing in his Pocket, had almoſt burnt its way out before it was perceived.

I am, &c.

VI. *An*

VII. *An Account of ſome Experiments on the Effects of the Poiſon of the* Rattle-Snake. *By Captain* Hall. *Communicated by Sir* Hans Sloane, Bar. Med. Reg. *&c.*

IN *South Carolina* on the 10*th* of *May*, *Anno* 1720, having got a fine healthful Rattle-Snake about four feet long, I perſwaded three or four Gentlemen, and one Mr. *Kidwell* a Surgeon, to aſſiſt me in making ſome Experiments on the Effects of its Poiſon.

We got three Curr-Dogs, the biggeſt not larger than a common Harrier, and the leaſt about the bigneſs of the largeſt ſiz'd Lap-Dog, all of them ſmooth haired.

The Snake being ty'd and pinn'd down to a Graſsplat, we took the largeſt of them, which was a white one, and having tied a Chord round his Neck, ſo that it ſhould not ſtrangle him, another Perſon held one end while I held the other; the length was not more than four Yards each way from the Dog.

Immediately on our bringing the Dog over the Snake, the Snake raiſed himſelf near two feet, and bit the Dog as he was jumping; the Dog yelpt, by which I perceived he was bitten; and upon it I pull'd him to me, as faſt as I could, and perceived his Eyes fixt, his Tongue between his Teeth, which were cloſed, his Lips ſo drawn up as to leave his Teeth and Gums bare: In ſhort, he was quite Dead in a quarter of a Minute; but one Perſon (beſide my ſelf) was of Opinion it was in half that time: The firſt was the Opinion of the By-ſtanders, who were five or

ſix; but I believe, none of them ſo much uſed to meaſure time as the Gentleman and I were, from our conſtant making uſe of the half Minute, and quarter Minute Glaſs at Sea. We could not ſee where the Dog was bitten, nor any Blood: Upon which we ordered ſome hot Water to ſcald the Hair off; when we could find but one Puncture, which look'd of a bluiſh Green a little round it; it was juſt between his fore Leg, and his Breaſt; where (when the Legs are diſtended) the Hair is much thinner than in ſome other places.

Half an Hour after the firſt Bite we took a ſecond Dog, which was ſomewhat leſs, of a Liver-Colour, and in like manner brought him over the Snake, which in a very little time bit his Ear, ſo that we all ſaw it; he yelpt very much, and ſoon ſhew'd the ſigns of being very ſick, holding that Ear that was bit uppermoſt. He reel'd and ſtagger'd about for ſome time; then he fell down, and ſtruggl'd as if Convulſed, and for two or three times got up, each time wagging his Tail, tho' ſlowly, and attempting to follow a Negro-Boy, who uſed to make much of him. We put him into a Cloſet, and order'd the Boy to look after him.

About an Hour after the ſecond was bitten, we took the third Dog in like manner: The Snake bit him on the right ſide of the Belly, about two Inches behind the long Ribs; for we ſaw he had drawn Blood there. The Dog at firſt, I mean for about a Minute, ſeem'd not to be hurt; ſo we let him go, being one we could get again when we pleaſed. For that Day we put up the Snake, imagining his Poiſon was very near, if not quite expended.

In a little time after, which was juſt two Hours after the ſecond Dog was bit, the Boy told us he was dead.

About

About an Hour after I perſwaded Mr. *Kidwell* to open him, and I was in no ſmall haſte to examine the Heart, where I perſwaded my ſelf, I ſhould diſcover ſomething extraordinary; but could not perceive any remarkable Difference between that and many others I had ſeen, where there was no Poiſon in the Caſe. Mr. *Kidwell* laid open the Skull, and was of Opinion, that the Brain was more red and ſwoln than any he had ever ſeen; and he told me a little while after, that the Blood turn'd very black.

For that Day we heard no more of the third Dog which was bitten; but the next morning the Woman who own'd him came to me, complaining of my Cruelty for killing her Dog. She did not know when he died, but ſaid ſhe ſaw him at ſeven that Evening, which was about 3 hours after he was bit; and that he was ſo ſick he could ſcarce wagg his Tail. None of theſe Dogs were ſwoln before they died.

On the *Saturday* following, which was the 14*th*, we got two Dogs both as big as common Bull-Dogs. The firſt Dog, which he bit on the Inſide of his left Thigh, died in half a minute exactly, in the Opinion of two Gentlemen, who kept their Watches in their hands all the while: There were two very ſmall Punctures in his Thigh, which lookt livid, tho' no Blood was drawn. This Dog did not ſwell for four hours after he was dead. I ſaw him, and order'd him to be bury'd.

The ſecond Dog was bit about an Hour after the firſt, on the out ſide of his Thigh, where we perceived the Blood at two places: He ſoon ſicken'd, and died in four Minutes.

We thought his Poiſon was not ſpent; ſo we got a Cat (for we could get no more Dogs) which he bit about an hour after, though I can't ſay where. The Cat was very ſick, and we put her up in a Cloſet: By ſome means the Cat was let out in leſs than an Hour and a half after ſhe was bitten. The next Morning early ſhe was found dead in the Garden, and much ſwoln; ſo that no body cared to examine or ſearch where ſhe was bit.

About a quarter of an Hour after he had bitten the Cat, he bit a Hen twice: The Hen ſeem'd very ſick and drooping, and could not, or did not fly up to her uſual place of Rooſt among the reſt that Night; but the next Day ſhe ſeem'd very well, and continued ſo till Evening, when I order'd her to be Killed, and her Fearhers ſcalded off: There were 2 Punctures in her Thigh, and a Scratch on her Breaſt over the Craw, all which lookt livid.

About a Week after, having got a large Bull-Frog, we brought that over him as uſual: He bit it with much force; ſo that he ſeem'd to faſten for a ſmall ſpace. The Frog died in two minutes or thereabouts. In leſs than a quarter of an Hour he bit a Chicken, which was hatcht the *Feburary* before, that died in 3 minutes; I can't ſay where it was bit, and I was at a loſs to try any further Experiments for a long time, for want of proper Subjects. Dogs and Cats were not to be had; for the good Women, whoſe Dogs had been killed, exclaimed ſo much, that I durſt not meddle with one afterwards.

About the Middle of *June* I took him out according to Cuſtom, and having got a common black Snake, not of the of Viper-kind, about two and a half or near three Feet long, in good Health,

juſt

juſt taken; I put them both together, and irritated them both, that they bit each other, and I perceived the black Snake had drawn Blood of the Rattle-Snake before I took them aſunder.

In leſs than 8 Minutes the black Snake was dead, and I could not perceive the Rattle-Snake at all the worſe or ſick.

On the laſt Day of *June*, being *Friday*, I took him out to try, whether if he bit himſelf, it would not prove mortal to him. I hang'd him ſo, that he was not above half his Length on the Ground; and with two Needles at the End of a Stick, one to prick, the other to ſcratch, irritated him ſo much that he ſoon bit himſelf, after having attempted to bite the Stick many times. I then let him down, and he was quite dead in 8 minutes or thereabouts, but am ſure it did not exceed 12 Minutes.

A Gentleman perſwaded me to cut the Snake in five Pieces which he gave to a Hog, the Head-part firſt, in Sight of many of us. The Hog eat up all the Snake, and 10 or 12 Days afterwards I ſaw the ſame Hog alive and in Health.

This was no more than I had ſeen before; but doubted there might be a Miſtake, as that they had taken ſome other Snake for a Rattle-Snake: For being at the Houſe of *Charles Hart* Eſq; they ſhew'd me a Snake, which a Negro told me he had kill'd juſt before; it was in three pieces, the Head of it bruiſed into the Ground. While I was looking on, a Sow came and eat it up very greedily, tho' the Negro-man endeavour'd to hinder her, being afraid it would kill her; for ſhe had Pigs following her.

I never heard she was ſick for it, tho' I inquired; and about ten Days after I ſaw her in very good Health. I have heard fifty Relations of the ſame kind, and am told that thoſe Hogs which feed in the Marſhes will run after the common ſort of Water-Snakes, which are not poiſonous, and will feed on them greedily: And I aſſure you, in *Maryland* laſt *Auguſt* was two years I ſaw a Hog eat up the Head of a Rattle-Snake juſt cut off, and while it was gaſping very dreadfully; and I was told, it was a common thing, and it would do them no harm.

On the 10*th* of *June* 1723, Mr. *Thomas Cooper*, a Gentleman who practiſes Phyſick at *Charles Town*, and who was late of *Wadham-College Oxon*, a very Ingenious Man, ſent to me to let me know, he had got a fine Rattle-Snake which had been taken not above 4 Days, was about three feet and a half long, and that he deſign'd to try whether he could ſave ſome of the Dogs after the Snake ſhould bite them. He provided a large quantity of *Venice-Treacle* or *Mithridate*, I can't poſitively ſay whether, which he divided into two Potions, each about two Ounces; to one of them he put a large quantity of *Diaphoretic Antimony*.

The firſt Dog which the Snake bit on the Inſide of the Thigh, died ſo ſoon *(viz.)* in about half a minute, that we could not get the Potion, which was that *without* Antimony, down his Throat ſoon enough to expect it could have Effect.

Above an Hour after, the ſecond Dog was bitten by him, and had two Punctures or Holes in the fleſhy part of the Inſide of his fore left Leg, which did bleed more than any I had ſeen before: We immediately got down his Throat that Preparation *with Antimony*. He ſoon grew very ſick and ſtrove to vomit,

vomit; but I think brought up very little, if any; he froth'd at the Mouth and bit at the Graſs, which he champed, as if he were mad; and indeed we were all afraid of him. We therefore put him into a Room and there kept him till next Morning, where I ſaw him as I thought recover'd: We throw'd him ſome Meat, which he eat, ſo we let him out and he went home. About a Month after that, the Dog's Hair came off, and his Maſter killed him, being ſo ugly to look at; he told me, he lookt like a Leprous Perſon, (that was his very Expreſſion;) I never heard this Dog ſwell'd.

The third Dog which he bit was a Shaggy Spaniel, about an hour and a quarter after the ſecond. He was bitten on the foremoſt part of his right Shoulder, as we perceived by the Blood. The Dog ſeem'd to bite at the place himſelf, and was very ſick for that whole Evening (*viz.*) about 2 or 3 hours; but without any means or application he recover'd, and I never heard he was ſick afterwards.

VIII. *Quædam Notabilia in Cerebris trium* Epilepſiâ *mortuorum obſervata, quibus accedit Hiſtoria de* ſubſtantiâ Cataractæ. *Epiſtola Domini* Waltheri Curieuſi Rhæti, *ad Dominum* J. F. Woolhouſe, F. R. S. &c.

CUM hiccè, (ubi per unum alterumvè Menſem dego) *Venetiis*, cadaveribus ſufficientibus aperiendis nobis ſit occaſio, apud virum clariſſimum *Santorinum Pr. Anat*, erat Vir 35. *Annorum*, temperamenti & habitûs gracilis, abhinc duobus annis correptus dolore grava-

gravativo capitis in parte anteriore versûs frontem & à D. Professoribus multa ac multa remedia accepit, sed frustra : transactis duobus annis Hæmorhagia narium Larga contigit, quæ tamen sponte item cessavit. Post Hæmorhagiam amisit sensum olfactorium, & posteà correptus fuit per duos annos *Paroxysmis Epilepticis* sæpiûs ingruentibus, denique mortuus est.

Secto cranio, vidimus Cerebrum in parte anteriore versùs *Duram Matrem* circà Regionem *Cristæ Galli*, in parte dextrâ durum & callosum, & arctissimè cum *Meninge durâ* connexum : in parte Cerebri anteriore versûs sinistram partem, inventa fuit extravasatæ materiæ sanguineæ aliqua quantitas, reliqua naturalia.

2. *Anasarcâ* laborans Mulier 60 annorum vehementissimis iisque sæpê repetitis acerbissimis *Insultibus* correpta obiit. Secto cranio, invenimus intrà *Piam Meningem* & Cerebrum, atque in anterioribus Cerebri ventriculis magnam satis extravasatæ lymphæ quantitatem, ut eâ ventriculi fermè turgidi fuerint. In *Plexibus Choroideis* conspectui sese tradidere plurima corpuscula albicantia, pellucida, serum aquosum in se continentia, variæ magnitudinis ; maxima ad magnitudinem seminis *Canabini* majoris accedebant, quæ à me, aliis dissentientibus, vesiculæ fuere nominata.

3. Dissecto Cranio Vetulæ alicujus quæ per multorum annorum tractum *Epilepsiæ* valdè obnoxia erat, vidimus intra *Piam Matrem* & Cerebrum, mediocrem Seri extravasati copiam, in anterioribus autem Cerebri ventriculis tanta talis Seri quantitas fuit, ut crepaturam illorum quasi minabatur. *Plexus Choroidæus* instar veri racemi apparebat uterque, vesiculæ inibi plurimæ, rotundæ, pellucidæ, & variæ magnitudinis nobis visui sese obtulêre, & instar elegantissimarum apparebant Perlarum.

Hiſt. Vetula circiter 50 annorum, dum viveret, ſiſtebat *Cataractam veram* in oculo ſiniſtro, Coloris *Perlacei*, magnitudine mediocris, ità tamen ut aliquantiſper pluſquàm dimidiam, eamque mediam *Pupillæ* partem occupabat, ſeu meliùs mediam & paulò pluſquàm dimidiam partem *Pupillæ*; colorem perlaceum habere repræſentabat, repræſentabat dico, nam vidi manifeſtè eam Cataractam retrò *Uveam*, & in 2dâ oculi Camerâ eſſe ſuſpicabar. Interrogatus à *Santorino*, in præſentiâ decem plùs minùs Medicorum, quid judicium meum ſit de hâc *Cataractâ*, an ſit *Cuticulæ* vel *Criſtallini* vitium? reſpondi, me potiùs pro *Cuticulâ* habere quam pro *Criſtallini* vitio, attamen id abſolutè & indubiè affirmare nolle, quoniam ſignum Pathognomonicum non datur. Reſpondenti mihi ridere cœpit, quod audeam aliquid de *Cuticulâ* ſive *membranulâ* ſtatuere, perſuaſus enim eſt ipſe, *Cl. Leiſteri Syſtema* certum eſſe *&c.* *Mortuâ diſſectâ*, hæc Vetula in oculo dicto ſiniſtro nobis de omnibus certiſſima obtulit: *Criſtallinus* enim *humor* erat pellucidus quibuſvis inſtitutis experimentis, nec ullâ maculâ aut minimâ obfuſcatione imbutus; erat equidem per totam ipſius molem ubique & æqualiter leviſſimè, leviſſimè inquam, citrino Colore perfuſus, vix namque aliquid hujus Citrini Coloris apparebat, in vivis adhuc dum exiſtente Vetulâ. *Cuticula* eadem Libera in nominatâ Camerâ exiſtebat, & non niſi duâbus ſubtiliſſimis fibrillis *Uveæ* adhærebat: Cætera erant naturalia, at *Pupilla* naturali quoque aliquantulùm major reperta; naturaliter ſivè à naturâ, & non præter-naturaliter. Hanc *Cataractam*, unà cum partibus vicinis omnibus, eo quo inveni ſitu monſtrare, & curioſorum oculos ſatiare poſſum; cujus ſectioni hâc *Die* 27 *Aprilis* hujus *Anni* 1727 factæ, reique ità repertæ Teſtes ſunt Cl. Dominus *Santorinus* & plures alii Medici & Chirurgi.

IX. *Two Chirurgical Queſtions, ſtated and anſwered by* John Douglas, *Surgeon, F. R. S.*

Q. I. WHether it is not poſſible in ſome Meaſure to relieve thoſe Perſons (who by reaſon of their great Age, bad Habit of Body, *&c.* cannot ſubmit to any of the great Operations for the Stone with tolerable hopes of Succeſs) by making an Artificial *Fiſtula* in the *Perinæum*?

A. That a great many Patients offer, both young and old, who are afflicted with the Stone in the Bladder, whom we cannot with any tolerable hopes of Succeſs, adviſe to ſubmit to the great Operations for the Stone, daily Experience ſhews. It is inhuman to cut them, and ſhews a very great Imperfection in our Art, to ſay we can give them no Relief. Is there no *Medium* yet found out between living in extream Miſery and ſubmitting to a deſperate Operation? Yes; *Thomas Fienús* about 125 Years ago, propoſed a Palliative Cure for ſuch Patients, where a radicative Cure could not be expected; an Operation which may be performed with ſafety on the moſt Ancient; the Wound is ſo ſmall, and the Parts cut of ſo little conſequence to Life. An Operation by which we can prevent or alleviate the moſt lamentable Effects of the Stone, *viz.* The total ſuppreſſion of Urine and the cutting Pains they endure in, and after making Water, *&c.* yet this Operation has been as little minded all this while by the *Hoſpital-*

Li-

Lithotomiſts, as *Roſſet*'s moſt excellent Treatiſe before I introduced the Hypogaſtrick Section in 1719. Tho' I admire *Fienús*'s deſign in making this *Fiſtula*, yet I can by no means approve of his way of doing it.

It ought to be performed thus: Place the Patient as in *Marianus*'s Operation; paſs a Staff into the Bladder, then cut the Skin and Fat until you lay that Part of the *Urethra* bare, which reaches from the *proſtate Gland* to the Cavernous *Urethra*; then make a ſmall Inciſion into it with the point of your Knife; then withdraw the Staff, and paſs a ſmall flexible *Canula* into the Wound of the *Urethra*; then dreſs the Wound S. A. extract the *Canula*, clean it and introduce it again every Dreſſing, that ſo you may leave a *Fiſtula* in the room of the Wound.

Through this *Fiſtula* the Patient himſelf or any one about him, may eaſily paſs an Oiled Probe, and puſh the Stone back whenever he finds himſelf attack'd with a ſuppreſſion of Urine, or when the Stone preſſes hard againſt the *Sphincter*, when endeavouring to make Water, which otherwiſe could not be done without the Ceremony of ſending for, and ſtaying in miſery till a Surgeon comes to paſs the *Catheter*, which in ſuch caſes is not always to be done, without a great deal of Pain, and ſometimes Danger. By this *Fiſtula* we can alſo very eaſily inject any Liquor that may be thought proper either to prevent or allay the Inflammation of the Bladder, or cleanſe it from the Gravel, or any other ſort of filth that may collect there, by which the increaſe of the Stone will be prevented, *&c.*

In Females all thoſe advantages are obtain'd by the natural ſtraitneſs and ſhortneſs of the *Urethra*, whence they never ſuffer the tenth part that Males do; which is an inconteſtable evidence that when the Paſſage into

the Bladders of Males is made as ſtreight and near as ſhort (as is done by the forementioned *Fiſtula*) they will reap the ſame advantages by it.

Therefore Artificial *Fiſtula*'s in the *Perinæum* ought to be made for thoſe who by reaſon of their great Age, and bad Habit of Body, *&c.* cannot undergo any of the great Operations for the Stone, with any hopes of Succeſs.

Q. 2. Whether it is not poſſible to dilate the artificial *Fiſtula* in the *Perinæum* of Males, and the *Urethra* of Females with Spunge or Gentian-Tents gradually increaſed for ſome time to ſuch a width, that we may eaſily paſs a pair of *Forceps* into the Bladder, with which the Stone when ſmall may be extracted, and when large, or of an irregular Figure, broke, and the pieces extracted gradually and at different times, when they cannot be extracted at once, without fatiguing the Patient too much?

A. To prove that both theſe *Fiſtula*'s may be dilated to a ſufficient bigneſs by the means propoſed, (eſpecially, if the parts are frequently bathed in a *Semicupium* or otherwiſe, as the Operator ſhall think proper, and ſome warm Oyl injected into the *Fiſtula* every time the Tent is changed, (the better to ſupple and relax the parts,) I ſhall only make uſe of three Arguments, *viz.* One from common Experience in Analogous Caſes; Another from the Operations of Nature on the ſame Parts; And the third from Inſtances of this Operation being perform'd after the Method I propoſe. 1*ſt*, Common Experience ſhews to what a great width Fiſtula's in all other parts of the Body, tho' very ſmall at firſt, may be dilated by Spunge or Gentian-Tents. 2*dly*, Nature herſelf with-

without any Art has frequently perform'd this Operation on both Sexes; in Males who have been cut for the Stone after the old way, and had *Fiſtula*'s remained in *Perinæo*. It is often found that ſome conſiderable time afterwards, Stones of no ſmall ſize have appeared which had made their way thro' the Sphincter of the Bladder into the Membranous *Urethra*, and ſtuck near the Orifice of the *Fiſtula*, whence they were eaſily and ſafely extracted. I ſaw a Stone lately as big as a Pullet's Egg that was expelled from the Bladder of a young Woman without any help, as her Phyſician aſſured me, and ſhe had no inconveniency afterwards, which certainly would have happen'd if it had been extracted after the common violent Method. In the laſt *Philoſophical Tranſaction*, Dr. *Beard* of *Worceſter* gives an Account of a yet larger Stone, that paſſed after the ſame manner, but ſhe had the common inconveniency afterwards, *viz.* an Incontinency of Urine, which was owing more to the roughneſs than the bigneſs of the Stone, which had lacerated the Parts as in the common Operation, which might have been prevented had ſhe been aſſiſted in time as above.

3*dly*. Monſ. *Collet* in his *Traité de la Taille*, gives an Account of a Gentleman on whom he perform'd this Operation three different times, and extracted in all ten Stones. His Words are as follow:

Enfin, Monſ. Uſſon *etant hors de danger & ſa playe prête a ſe cicatriſer, prévoyant bien ce qui pourroit arriver dans la Suite, je lui conſeillai de tenir ſa playe ouverte, pour y entretenir ſeulement une petite Canule, qui dans l'occaſion donneroit une entiere liberté de faire Injection pour nettoyer la Veſſie de toutes les Immondices & pour qu'on pût enlever les nouvelles Pierres qui pourroient ſe former. Il prit ce parti, & il ſe trouva bien durant cin-*

cinque Années, quil a Vecu depuis avec aſſez de Santé Mais a trois differents repriſes, je me ſuis vû Obligè de lui tirer juſqu'a dix autres pierres qui ſ'etoient formées dans laVeſſie : une petite Tente d'eponge preparée miſe pendant quelques heures dans la Fiſtule à la place de la Canule me facilitoit l'Entrée d'une tres petite tennette. Monſ. Uſſon *s'habilloit dans ce Moment & il ſortoit pour aller par tout ou ſes Affaires l'appelloient.*

I know a Gentleman that now keeps a *Fiſtula in Perinæo* open for the very ſame reaſons.

Since then it is evident that *Fiſtula*'s in all parts of the Body are dilatable to a great width, ſince Nature is often able of her ſelf to dilate the very parts in diſpute, to a very extraordinary Degree; and ſince this very Operation I here propoſe has been ſucceſsfully performed three times on the ſame Perſon,

Therefore *Artificial Fiſtula*'s in Males and the *Urethra* of Females may be dilated ſo as to extract any Stone without cutting the Body of the Bladder, or lacerating any of the Parts.

N. B. This Operation will appear much leſs ſurprizing, if we conſider, that it is only dilating the Sphincter of the Bladder, *&c.* in a different manner from that daily done in *Marianus*'s Operation, *i. e.* By this Method the Parts are dilated gently and gradually, after being well bath'd and anointed, the better to relax them. Whereas in *Marianus*'s Operation they are dilated quickly and violently, and then the Stone, be it of what Largeneſs or Figure ſoever, is forcibly drawn out immediately; whence Contuſions, Lacerations, Hæmorrhages, Inflammations, Mortifications, *&c.*

X. *The Concluſion of Dr.* Deſagulier's *Account of* Mr. Hales's *Vegetable Staticks.*

CHAP. VII. *Of Vegetation.*

OUR Author in this Chapter applies his ſeveral Experiments, and Concluſions drawn from them, to Vegetation; and ſhews chiefly the following Things, *viz.* That Vegetables are compos'd of Sulphur, volatile Salt, Water, Earth and Air.

That in Nutrition the Sum of the attracting Powers of thoſe Subſtances is ſuperior to the Sum of the repellent; and as the watry Vehicle flies off, the Parts harden.

That Oil, which is made up of Sulphur and Air, abounds in Seeds for their better preſervation.

That in cold Countries, where thoſe Principles are not ſo firmly united, ſmall Wines, ſuch as *Rheniſh*, moſt eaſily yield their Tartar (which by Experiments appears to contain Oil and Air;) but generous Wines, ſuch as *Madera*, having thoſe Principles more firmly united, will bear a great degree of Heat before they part with them.

That the uſe of the Leaves of Trees is to bring up Nouriſhment within reach of the Attraction of the Fruit, to carry off the redundant watry Fluid; to imbibe Rains and Dews which are impregnated with Salt and Sulphur; as likewiſe to imbibe Air, and to be of the ſame uſe to Plants as the Lungs are to Animals.

That Plants which are overſhaded, or too replete with Moiſture, cannot ſo well imbibe Air: Therefore, tho' they will ſhoot out faſt, and have much Wood, they will be more barren in proportion.

Mr.

Mr. *Hales*, by a very ingenious Contrivance, found the Degrees of growing in every part of young Shoots, which in their growing extend themſelves moſt in the middle, and leaſt towards the top and the bottom; the ductile Matter for their growth being drawn out in length like melted Glaſs-Tubes, which retain a Hollowneſs, thò' drawn out to the ſmalleſt Thread. N.B. *In ſome Animals there is ſuch a tough ductile Subſtance, which hardens when expos'd to the Air in ſmall Threads, as in Spiders and Silk-Worms.*

He ſhews, that the Pith ſerves to ſupply the dilating Moiſture for the tender Shoots; but that their Figure may be oblong, and not round, as the Fruit commonly is, there are tough Diaphragms in the Pith at ſmall diſtance from each other, which check the lateral Expanſion; as alſo horizontal Fibres, which ſerve for the ſome purpoſe: And of the ſame ſort is the Pith in the large growing Feathers of Birds; which is made up of Veſicles that can be diſtended lengthwiſe, but have Sphincters at the ends, to prevent too large a lateral Dilatation. — That the Bones of Animals do not grow at the Joynts (which would prevent their free Motions) but at the *Symphyſis*, viz. where the Heads joyn to the Shanks of the Bones — That there are particular Veſſels in Vegetables, as well as Animas, appropriated for conveying different ſorts of Nutriment; and that where a viſcid Subſtance is to befurniſhed, the Veſſels are lengthen'd, and often fetch a compaſs to retard the Velocity of the Fluid, which is to be inſpiſſated into an hard Subſtance. Thus in hard Stone-Fruits the Umbilical Veſſel goes round theConcave of the Stone, and then enters the Kernel near its Cone.

Then at laſt, our Author traces the Vegetation of a Plant, from a Seed to a Tree again producing Seed; which

which Account, as it cannot well be contracted, without leaving out ſomething material; and as it ſerves to ſhew ſome of the excellent uſes of our ingenious Philoſopher's happy Diſcoveries, I ſhall (to conclude this Abſtract) give in the Author's own Words.

" Supported by the Evidence of many of the fore-
" going Experiments, I will now trace the Vegetation
" of a Tree, from its firſt ſeminal Plant in the Seed,
" to its full Maturity and Production of other Seeds,
" without entring into a particular Deſcription of the
" Structure of the Parts of Vegetables; which has
" already been done by Dr. *Grew* and *Malpighi*.

" We ſee by *Experiment* 56, 57, 58, on diſtilled
" Wheat, Peaſe, and Muſtard-Seed, what a wonderful
" Proviſion Nature has made, that the Seeds of Plants
" ſhould be well ſtored with very active Principles;
" which Principles are there compacted together by
" him, who curiouſly adapts all things to the pur-
" poſes for which they were intended, with ſuch a juſt
" degree of Coheſion, as retains them in that State,
" till the proper Seaſon of Germination: For if they
" were of a more lax Conſtitution, they would too
" ſoon diſſolve like the other tender Annual Parts of
" Plants: And if they were more firmly connected,
" as in the Heart of an Oak, they muſt neceſſarily
" have been many Years in germinating, tho' ſupplied
" with Moiſture and Warmth.

" When a Seed is ſown in the Ground, in a few
" Days it imbibes ſo much Moiſture, as to ſwell with
" very great force; as we ſee in the Experiment on
" Peaſe in an Iron-Pot. This forcible Swelling of the
" Lobes of the Seed *a r*, *a r*, (Fig. 1.) does probably
" protrude Moiſture and Nouriſhment from the capil-
" lary Veſſels *r r*, which are called the Seed-Roots,

" into the Radicle *c z d*; which Radicle, when it is thus
" ſhot ſome length into the Ground, does then im-
" bibe Nouriſhment from thence; and after it has ac-
" quir'd ſufficient Strength, as this tender ductile Root
" is extending from *z* to *c*, it muſt neceſſarily carry
" the expanding Seed-Lobes upwards at the ſame time
" that the dilating from *z* to *d* makes it ſhoot down-
" wards; and when the Root is thus far grown, it
" ſupplies the Plume *b* with Nouriſhment; which
" thereby ſwelling and extending, opens the Lobes
" *a r*, *a r*, which are at the ſame time raiſed above-
" ground with the Plume *i*, where they by expanding
" and growing thinner, turn to green Leaves (except
" the Seeds of the Pulſe-kind;) which Leaves are of
" ſuch importance to the yet tender Plume, that it
" periſhes, or will not thrive if they are pulled off:
" Which makes it probable, that they do the ſame Of-
" fice to the Plume that the Leaves adjoining to Ap-
" ples, Quinces and other Fruit do to them, *viz.* they
" draw Sap within the reach of their Attraction. (See
" *Exper.* 8. and 30.) But when the Plume is ſo far
" advanced in growth, as to have Branches and ex-
" panded Leaves to draw up Nouriſhment, then theſe
" ſupplemental ſeminal Leaves *a r*, *a r*, being of no
" farther uſe, do periſh; not only becauſe the now-
" grown and more expanded Leaves of the young
" Plant or Tree do ſo overſhade the ſupplemental
" Leaves, that the former more plentiful Perſpiration
" is much abated; and thereby alſo their power of
" attracting Sap fails; but alſo becauſe the Sap is
" drawn from them by the Leaves; and they being
" thus deprived of Nouriſhment, do periſh.

" As the Tree advances in ſtature, the firſt, ſecond,
" third and fourth Order of the lateral Branches ſhoot
" out, each lower Order being longer than thoſe imme-

" diately

" diately above them; not only on the account of
" Primogeniture, but alſo becauſe being inſerted in
" larger parts of the Trunk, and nearer the Root,
" they have the advantage of being ſerved with great-
" er plenty of Sap, whence ariſes the beautiful para-
" bolical Figure of the Trees.

" But when Trees ſtand thick together in Woods or
" Groves, this their natural Shape is alter'd; becauſe
" the lower lateral Branches being much ſhaded, they
" can perſpire little; and therefore drawing little Nou-
" riſhment, they periſh: But the Top-Branches being
" expoſed to a free drying Air, they perſpire plenti-
" fully; and thereby drawing the Sap to the Top,
" they advance much in height. But *vice verſâ*, if
" when ſuch a Grove of tall Trees is cut down, there
" be left here and there a ſingle Tree, that Tree will
" then ſhoot out lateral Branches; the Leaves of which
" Branches now perſpiring freely, will attract plenty
" of Sap; on which account, the Top being deprived
" of its Nouriſhment, it uſually dies.

" And as Trees in a Grove or Wood grow only in
" Length, becauſe all the Nouriſhment is by the Leaves
" drawn to the top, moſt of the ſmall lateral ſhaded Bran-
" ches in the mean time periſhing for want of Perſpira-
" tion and Nutrition: So the caſe is the very ſame in
" the Branches of a Tree, which uſually making an An-
" gle of about 45 Degr. with the Stem of the Tree, do
" thereby beautifully fill up at equal and proper Diſtan-
" ces the ſpace between the lower Branches and the top
" of the Tree, forming thereby as it were a parabolical
" Grove or Thicket; which ſhading the Arms, the
" ſmall lateral Shoots of thoſe Arms uſually periſh for
" want of due perſpiration; and therefore the Arms
" continue naked like the Bodies of Trees in a Grove;
" all the Nouriſhment being drawn up to the tops of

" the ſeveral Branches by the Leaves which are there
" expos'd to the warm Sun and free drying Air, where-
" by the Branches of Trees expand much.

" And where the lateral Branches are very vigo-
" rous, ſo as to make ſtrong ſhoots, and attract the
" Nouriſhment plentifully, there the Tree uſually a-
" bates of its height: But where the Tree prevails in
" height, as in Groves, there commonly its lateral
" Branches are ſmalleſt. So that we may look upon a
" Tree as a complicated Engine, which has as many
" different Powers as it has Arms and Branches, each
" drawing from their common Fountain of Life the
" Root: And the whole of each yearly growth of the
" Tree will be proportionable to their attracting Pow-
" ers, and the Quantity of Nouriſhment the Root af-
" fords. But this attracting Power and Nouriſhment
" will be more or leſs, according to the different Ages
" of the Tree, and the more or leſs kindly Seaſons of
" the Year.

" And the proportional growth of their lateral and
" Top-Branhes in relation to each other, will much
" depend on the difference of their ſeveral attracting
" powers. If the Perſpiration and Attraction of the
" lateral Branches is little or nothing, as in Woods and
" Groves, then the Top-Branches will mightily pre-
" vail; but when in a free open Air the Perſpiration
" and Attraction of the lateral Branches come nearer
" to an Equality with that of the Top, then is the a-
" ſpiring of the Top-Branches greatly check'd. And
" the caſe is the ſame in moſt other Vegetables, which
" when they ſtand thick together, grow much in length
" with very weak lateral Shoots.

" And as the Leaves are thus ſerviceable in promo-
" the growth of a Tree, we may obſerve, that Nature
" has plac'd the Pedals of the Leave-ſtalks where moſt

" Nou-

" Nouriſhment is wanting to produce Leaves, Shoots
" and Fruit; and ſome ſuch thin leafy Expanſion is
" ſo neceſſary for this purpoſe, that Nature provides
" ſmall thin Expanſions, which may be called prima-
" mary Leaves that ſerve to protect, and draw Nou-
" riſhment to the young Shoot and Leaf-buds before
" the Leaf it ſelf is expanded.

" And herein we ſee the admirable Contrivance of
" the Author of Nature, in adapting her different
" Ways of conveying Nouriſhment to the different
" Circumſtances of her Productions. For in this *Em-*
" *bryo*-ſtate of the Buds a ſuitable proviſion is made
" to bring Nouriſhment to them in a Quantity ſuffici-
" ent for their then ſmall Demands. But when they
" are in ſome degree increaſed and formed, a much
" greater quantity of Nouriſhment is neceſſary in pro-
" portion to their greater Increaſe: Nature, that ſhe
" may then no longer ſupply with a ſcanty hand, im-
" mediately changes her Method, in order to convey
" Nouriſhment with a more liberal hand to her Pro-
" ductions; which Supply daily increaſes by the grea-
" ter Expanſion of the Leaves, and conſequently the
" more plentiful Attraction and Supply of Sap, as the
" greater growth and demand of it increaſes. We find
" a much more elaborate and beautiful *Apparatus* for
" the like purpoſe, in the curious Expanſions of Bloſ-
" ſoms and Flowers, which ſeem to be appointed by
" Nature, not only to protect, but alſo to draw and
" convey a Nouriſhment to the *Embryo*-Fruit and
" Seeds. But as ſoon as the *Calix* is form'd into a
" ſmall Fruit now impregnated with its minute ſemi-
" nal Tree, furniſh'd with its *Secondine*, *Chorion* and
" *Amnion* (which new-ſet Fruit may in that ſtate be
" look'd upon as a compleat Egg of the Tree, yet in
" Embryo) then the Bloſſom falls off, leaving this

new-

" new-form'd Egg or first-set Fruit in this Infant-state,
" to imbibe Nourishment sufficient for it self, and the
" *Fœtus* with which it is impregnated : Which Nou-
" rishment is brought within the reach and power of
" its Suction by the adjoyning Leaves.

" If I may be allow'd to indulge Conjecture (in a
" Case, in which the most diligent Inquirers are as
" yet, after all their laudable Researches, advanced
" but little farther than meer Conjecture) I would
" propose it to their consideration, whether from the
" manifest Proof we have that Sulphur strongly at-
" tracts Air, a hint may not be taken to consider whe-
" ther this may not be the primary use, of the *Farina*
" *fœcundans*, to attract and unite with it self elastick
" or other refin'd active Particles.

" That this *Farina* abounds with Sulphur, and that
" a very refin'd sort, is probable from the subtil Oil
" which Chymists obtain from the Chives of Saffron.
" And if this be the use of it, was it possible
" that it could be more aptly placed for the
" purpose on very movable *Apices* fixed on the slender
" Points of the *Stamina*, whereby it might easily with
" the least breath of Wind be dispersed in the Air,
" thereby surrounding the Plant as it were with an At-
" mosphere of sublimed sulphureous Pounce ? For many
" Trees and Plants abound with it ; they may be per-
" haps inspir'd at several parts of the Plant, and espe-
" cially at the *Pistillum*, and be thence conveyed to
" the *Capsula seminalis*, especially towards Evening;
" and in the Night, when the beautiful *Petala* of the
" Flowers are closed up, and they, with all the other
" parts of the Vegetable, are in a strongly imbibing
" state. And if to these united sulphureous and aerial
" Particles we suppose some Particles of Light to be
" joyned (for Sir *Isaac Newton* has found that Sulphur

" attracts

" attracts Light ſtrongly) then the reſult of theſe
" three by far the moſt active Principles in Nature,
" will be a *Punctum Saliens* to envigorate the *Seminal*
" Plant: And thus we are at laſt conducted by the
" regular Analyſis of vegetable Nature to the firſt
" enlivening Principle of their minuteſt Origin.

P. S. *Since I began this Abſtract, I was with the ingenious Author, who by ſome Experiments try'd ſince the writing of his Book, was become certain of ſome things which he ſpoke doubtfully of before,* viz, 1. *That the Diaphragm-Inſtrument* (*ſee Page* 263, *of his Book*) *wou'd ſerve to breathe* 8 ½ *Minutes when it was entirely dry, and the Diaphragms impregnated with Salt of Tartar.* 2. *That Leaves imbibe Air, as he has ſince tried in Mint in the manner that he mention'd* (*in Page* 329.) 3. *That this year the Heat of the Weather has been at* 84 *Degrees, mark'd on his Thermometers.*

F I N I S.

Advertiſement.

WHereas I publiſh'd a Paper concerning the Figure of the Earth, in the *Philoſophical Tranſactions*, Numb. 388, and a Paragraph which I cancell'd, was, by the miſtake of my *Amanuenſis*, printed whilſt I was ſick: I deſire ſuch Readers as have that *Tranſaction* by them, to cancel from the Words, *Laſtly, Let us ſuppoſe*, Page 280. Lin. *ult.* to the End of the Paragraph.

Having from time to time neglected to give this notice, I choſe to do it here, tho' late, rather than wholly to omit it.

J. T. Deſaguliers.

Jos: Banks

Numb. 400.

PHILOSOPHICAL TRANSACTIONS

For the Months of *October*, *November*, and *December*, 1727.

The CONTENTS.

I. *Obſervatio* Solaris Deliquii *celebrati Die* 25. *Septemb.* 1726. *habita* Ulyſſipone *in Obſervatorio Regii Palatii* A' P. Jo. Baptiſta Carbone, *Soc Jeſ.*

II. Lunaris Eclipſis *celebrata die* 10. *Octob. an.* 1726. *& in Obſervatorio Collegii D.* Antonii Magni *obſervata ab* Eodem.

III. *Remarks upon ſome Experiments in* Hydraulics, *which ſeem to prove, that the Forces of equal moving Bodies are as the Squares of their Velocities. By Mr.* John Eames, F. R. S.

IV. *A Copy of a Letter from Dr.* David Kinneir, Coll. Med. Edingb. Soc. *to Dr.* Campbell, Coll. Med. Lond. Soc. Honorar. *touching the Efficacy of* Camphire *in* Maniacal *Diſorders.*

V. *A Method for determining the Geographical Longitude of Places, from the Appearance of the common Meteors, called* Falling Stars, *propoſed by* George Lynn, *Eſq; of* Southwick, Northampton-ſhire, *in a Letter to Dr.* Jurin, R. S. & Coll. Med. Soc.

The CONTENTS.

VI. *An Attempt made before the* Royal Society, *to ſhew how Damps, or foul Air, may be drawn out of any Sort of* Mines, &c. *by an* Engine *contriv'd by the Reverend* J. T. Deſaguliers, L. L. D. *and* F. R. S.

VII. *A Letter to* James Theobald, *Eſq; giving an Account of the* Norwegian Finns, *or* Finlanders, *by* Peter Kinck, *Eſq;*

VIII. *A preternatural Perforation found in the upper Part of the* Stomach, *with the* Symptoms *it produc'd, by Mr.* Chriſtopher Rawlinſon, *Surgeon.*

IX. *An Account of an Humane Skeleton of an extraordinary Size, found in a Repoſitory at* Repton *in* Derby-ſhire, *together with ſome Examples of long Life, communicated by* Simon Degg, M. D. & F. R. S.

X. *A farther Account of the Bills of Mortality,* &c. *of ſeveral conſiderable Towns in* Europe, *for the Years* 1722 *and* 1723, *extracted from the* Acta Breſlavienſia, *by Sir* Conrad Spengell, R. S. S. & Col. Reg. Medic. Lond. Lic.

XI. *Ingens Materiæ Purulentæ copia cariem & ſeparationem totius* Oſſis Mali, *&c. inducens, & per triennium ex Ore Pueri defluens, obſervata à Domino* Petro Hardiſway, M. D.

I. *Ob-*

I. *Obſervatio* Solaris Deliquii *celebrati Die* 25. *Septemb.* 1726. *habita* Ulyſſipone *in Obſervatorio Regii Palatii* A' P. Jo. Baptiſta Carbone, *Soc. Jeſ.*

COElo, præter ſpem, clariſſimo Déliquium hoc Ulyſſipone conſpeximus, ejuſque finem tantum non vidimus; monte ſiquidem interjecto, delituit Sol uno circiter minuto temporis antequam plenè reſtitueretur. Ad id ego obſervandum Teleſcopio uſus ſum pedum 8. Pariſinorum, quod micrometro inſtruxeram accuratiſſimè elaborato. Nonnulla tamen obſervanti acceſſerunt incommoda, quibus præpedientibus, macularum Solarium (quamplures eo die videbantur in Sole) Immerſiones, atque Emerſiones, quin & phaſes aliquot digitorum inveſtigare, aut opportunè adnotare non licuit. Quas tamen obſervare datum eſt, eas ſanè ſenſibili errore carere autumo; tum quòd in digitis Solaribus rectè dimetiendis nullam prætermiſſerim diligentiam; tum quod correctionem temporis (cui potiſſimum nititur hujuſmodi Obſervationum veritas) repetitis eo die Obſervationibus accuratiſſimè inveſtigaverim. Ne verò ſuo careat teſtimonio prædicta correctio, duas hîc apponam altitudines Solis, quarum alteram ante Eclipſeos initium, alteram ſub ejus finem, Quadrante Aſtronomico trium pedum Pariſinorum, deprehendi; & utraque ſanè Obſervationi Meridianæ conſentit, ejuſdemque defectûs horologium arguit.

	H.	′	″
In Meridie verò, ſeu Apparenti indicabat horologium. - - - - - - -	11	58	34
Nempe deficiebat à Tempore vero - -		1	26

	°	′	″
A' Meridie; Altitudo vera centri Solis	28	53	45
Ejuſdem Declinatio Auſtralis - - -		53	18
Altitudo Poli, ex pluribus Obſervationibus, certa, ſaltem quoad minuta prima	38	42	0
Quibus datis invenitur per Trigonometriam arcus diſtantiæ Solis à Meridiano	50	49	36
	H.	′	″
Qui, ſi in tempus convertatur, dat - -	3	23	18
Indicabat verò horologium - - - -	3	21	48
Ergo à tempore vero deficiebat - - -		1	30

	°	′	″
Iterum, Altitudo vera centri Solis - -	4	52	54
Ejuſdem Declinatio Auſtralis - - -		55	24
Altitudo Poli, ut ſupra - - - -	38	42	0
Ex quibus denuò per Trigonometriam infertur arcus diſtantiæ Solis à Meridiano	82	59	40
	H.	′	″
Qui in tempus converſus, dat - -	5	31	59
Indicabat verò horologium - - -	5	30	26
Ac proinde à tempore vero deficiebat - -		1	33

Hanc igitur æquationem tempori horologii prudens addidi, quò fieret tempus verum, ſeu apparens correctum, eoque uſus ſum in ſequentibus Obſervationibus.

	Temp. Ver. H.	′	″
Incipit limbus Lunæ limbum Solarem perſtringere. *dub.* - - - - - - -	5	59	50
I. Digitus latet - - - - - - -	4	5	10
II. Digitus - - - - - - - - -	4	10	45
III. Digitus - - - - - - - - -	4	16	55
Dimidium quarti - - - - -	4	20	14
IV. Digitus - - - - - - - - -	4	23	48

V. Di-

	H.	′	″
V. Digitus	4	31	5
Dimidium ſexti	4	34	51
VI. Digitus	4	38	57
VII. Digitus	4	47	57
Dimidium octavi	4	54	59
Maximæ obſcurationis tempus, quantum ex reliquis phaſibus colligi poteſt	4	58	30
Ejuſdem verò Quantitas, dig. 7. min. 45. nempè revolutiones cochleæ 11. & $\frac{13}{20}$, quarum 18. integras præcontinebat Diameter Solis.			

EMERSIO.

	H.	′	″
Dimidium ſeptimi digiti latet	5	13	40
VI. Digitus	5	17	58
V. Digitus	5	25	58
Dimidium quinti	5	29	43
Dimidium quarti	5	36	49
III. Digitus	5	40	8
Dimidium tertii	5	43	14
II. Digitus	5	46	10
Dimidium ſecundi	5	48	58
I. Digitus	5	51	40
Limbus Solis inferior montem ſtringit.	5	52	42
Sol totus deliteſcit, adhuc deficiens min. circiter 12. nempè $\frac{1}{5}$ unius digiti.	5	55	27

	H.	′	″
Altitudo apparens montis, ſeu limbi ſuperioris Solis, 57′ 30″, quæ correcta, erit 34′ 29″; ex his rurſus ſubtractâ quantitate ſemidiametri Solaris 16′ 4′ remanebit, altitudo vera centri Solis, quando totus delituit 18′ 25″; ex qua, reliquis etiam datis, infertur per Trigonometriam hora prædictæ altitudini reſpondens, nimirùm - - - - - - - - -	5	55	27
Occaſus Solis verus - - - - - -	5	57	1
Sed apparens, propter refractionem, circ.	6	0	0
Unde patet, finem Eclipſeos in noſtro Hemiſpherio viſibilem fuiſſe; contigit enim, quantùm ex prædictis colligi poteſt, circiter. - - - - - - - - -	5	56	50

II. Lunaris Eclipſis *celebrata die* 10. *Octob. an.* 1726. *& in Obſervatorio Collegii D.* Antonii Magni *obſervata ab* Eodem.

INnumeris ea nocte vaporibus inficiebatur aër, qui ſpiſſiores in nubes paulatim denſabantur, ipſamque Lunam non mediocriter offuſcabant. Verebar proinde, ne non bene mihi, ac feliciter Lunaris Defectus Obſervatio contingeret; futurum enim obnubilum tota ea nocte cœlum videbatur. Sub mediam verò noctem, Euro-Borea ſpirante, ſenſim aër diverberari, mox etiam diſpergi nubes viſæ ſunt; atque horâ circiter ſecundâ Occidentalis plaga, ad quam Luna vergebat, nitidiſſima apparuit, eademque ad finem Eclipſeos permanſit. Nullo

igitur

igitur aëris incommodo Obſervationem hanc habui; nec quidquam prætermiſi, quod ejus rectitudini favere quoque modo intelligerem. Eodem uſus ſum Teleſcopio, quo in Obſervatione Solaris Deliquii; idemque adhibui micrometrum in definiendis Digitis Obſcurationis.

Ad Temporis verò dimenſionem horologio uſus ſum, pendulo inſtructo; cujus tanta æqualitas motus, ut dierum intervallo decem, & octo, vix uno, aut altero ſecundo à Medio Solis motu diſcrepaſſe deprehenderim; idque accuratiſſimo, ac quotidiano penè examine per appulſum fixæ ad eundem verticalem. Ejus differentiam à Tempore Vero, ſeu Apparenti repetitis interdiù Obſervationibus inveſtigavi, tùm ſcilicet per Solis altitudines, tum per lineam Meridianam; quas tamen Obſervationes libens prætereo, ne longior ſim; ejuſmodi enim ſunt, atque exhibitæ in præcedenti Solaris Eclipſeos Obſervatione.

Macularum nomina ex Selenographia P. Grimaldi excerpta ſunt, quam P. Ricciolius ſuo Almageſto novo inſeruit, ac nominibus locupletavit. In eorum verò commodum, qui Hevelianam ſequuntur Nomenclaturam, ſynonymas quoque voces, ex hoc Authore deſumptas, ſecundo loco apponam, præpoſita litera H, quæ Hevelium ſignificat.

	Temp. V. cor. H. ′ ! ″
Incipit penumbra ſenſibilis limbum Lunæ ad Euro-Auſtrum inficere. - -	14 37 0
Spiſſior apparet - - - - - - - - -	14 46 0
Spiſſiſſima - - - - - - - - - - -	14 56 0
Umbra Terræ, quantum diſcerni poteſt, ad eundem limbum pertingit - -	14 57 20
Schikardus in Umbra - - - - - -	15 3 50

	h	′	″
I. Digitus obumbratur - - - - -	15	4	53
Umbra ad *Kriſtmanùm* - -	15	8	4
Immergitur totus - - - - - -	15	9	15
Merſenius latet: Litus Orientale *Maris humorum* incipit obumbrari: H. *Sinus Sirbonis* - -	15	9	50
II. Digiti latent - - - - - - -	15	13	0
Capuanus immergitur - -	15	13	36
Ad *Grimaldum* pervenit Umbra: H. *Pal. Mareotis* - - -	15	14	54
Gaſſendus incipit obumbrari -	15	17	34
Incipit *Tycho*: H. *Mons Sinai*. *Grimaldus* latet. - - - - -	15	19	46
Tychonis medium tenet Umbra.	15	21	0
III. Digiti. *Tycho* totus immergitur -	15	21	53
Morinus: H. *Caſſius* - -	15	23	58
Bullialdus H. *Inſula Creta* -	15	25	20
Profatius incipit obumbrari -	15	30	58
IV. Digiti - - - - - - - - -	15	31	48
Profatius latet. - - - - -	15	32	40
V. Digiti - - - - - - - - -	15	43	40
Umbra tantum non attingit *Galilæum*, quem propter diutiſſimè verſatur - - - - - - -	15	48	0
Ad *Snellium*, & *Fornerium* pervenit Umbra - - - - -	15	53	20
Ad *Fracaſtorium*. *Snellius* & *Fornerius* latent - - - - -	15	55	38
Fracaſtorius totus in Umbra -	15	58	30
VI. Digiti - - - - - - - - -	16	3	4
Sinum Æſtuum tangit Umbra: H. *Mare Adriaticum* - - -	16	6	13

Medium

	H.	′	″
Medium *Vendelini* - - -	16	8	22
Totum *Mare Nectaris* in Umbra. H. *Sinus extremus* - -	16	14	40
Grimaldus incipit emergere -	16	15	51
Ricciolius extra Umbram - -	16	16	18
Grimaldus totus - - - -	16	23	30
Umbra ad *Langrenum* - -	16	24	27
Gaſſendus reſtituitur - - -	16	43	34
V. Digiti deficiunt - - - - -	16	47	22
Bullialdus incipit emergere -	16	51	57
Totus extra Umbram - - - -	16	53	47
Kriſtmanus incipit - - -	16	54	17
Item *Schikardus* - - - -	16	58	0
IV. Digiti. Totus *Schikardus* - -	16	59	27
Pitatus totus - - - - -	17	3	30
III. Digiti - - - - - - - - -	17	9	20
Tycho incipit emergere - -	17	10	54
Tychonis medium extra Umbram	17	11	49
Tycho totus emergit - - -	17	12	40
Fracaſtorius totus - - - -	17	14	20
II. Digiti - - - - - - - - -	17	18	4
Snellius emergit - - - -	17	25	43
I. Digitus tantum latet - - - -	17	26	10
Fornerius extra Umbram - -	17	27	55
Extremus limbus Lunæ, qui ad Africum ſpectat videtur proximè emerſurus - - - - - - - -	17	32	50
Jam Lunæ diſcus integer appareret, ac proinde Finis Eclipſeos	17	33	30
Adhuc denſiori penumbra prædictus limbus inficitur - - -	17	38	0

Deſinit

	H.	′	″
Deſinit penumbra ſenſibilis, ſuæque Luna claritati omninò reſtituta apparet - - - - - - - - -	17	54	0

	H.	′	″
Ex obſervatis Initio, ac Fine Eclipſeos colligitur ejus Duratio - - - - -	2	36	10
Medium verò, ſeu Maxima obſcuratio.	16	15	25
Quantitas Micrometro diligenter inveſtigata, Dig. 6. min. 10′. - - -			

Terreſtris Umbra in hac Eclipſi tum malè terminata, quod in cæteris Lunæ Eclipſibus contingere ſolet, tum etiam ſubluſtrior, atque aliàs viſa eſt. Difficilior proinde veri Initii, Finiſque inveſtigatio fuit; nec ſuo carituras arbitror diſſidio diverſas Aſtronomorum Obſervationes, qui ſcilicet in eodem penumbræ termino definiendo perquam rarò conveniunt; ac tum vel maximè, quando nulla Terreſtris Umbræ portio in diſco Lunari verſatur, uti nulla verſari incipiente Eclipſi, vel deſinente poteſt. Reliquæ verò phaſes, faciliùs, ac citra ſenſibilem errorem determinari potuerunt, comparatione habita inter Umbram veram, ac ſpiſſiorem penumbram. Equidem, ut rectitudini Obſervationis, quoad fieri poterat, diligentiſſimè conſulerem, plura ſub Eclipſeos Initium, plura ſub ejus Finem momenta temporis adnotavi; quorum ea tandem vero Initio, ac Fini adſcripſi, quæ utrique maximè congruere ex pluribus circunſtantiis deprehendi.

III. *Re-*

III. *Remarks upon ſome Experiments in* Hydraulics, *which ſeem to prove, that the Forces of equal moving Bodies are as the Squares of their Velocities.* *By Mr.* John Eames, F. R. S.

THE Reſult and Subſtance of theſe Experiments is, That the Velocities of any Fluid, for Inſtance Water, iſſuing out at equal Orifices made in the Sides of Tubes, or Veſſels filled up to different Heights, and kept full at thoſe Heights, above the Orifices, are found to be as the ſquare Roots of thoſe Heights reſpectively. Thus, when the different Heights above the Orifices are as the Numbers 1, 4, 9, 16, *&c.* the Velocities of the Particles of Water, iſſuing out, are found to be as the Numbers 1, 2, 3, 4, *&c.*

The Argument drawn from theſe Experiments, in Favour of the Opinion, that the Forces of equal Maſſes, or moving Bodies, are proportional to the Squares of their Velocities, runs thus. All the Particles of Water, being of the ſame Nature, and uniform, every ſingle Particle iſſuing out with Two Degrees of Velocity, muſt move with 4 Times the Force of any other ſingle Particle, that moves but with One Degree of Velocity; becauſe the Force with which it moves, is the Effect of a Cauſe 4 Times greater; namely, the Preſſure of a Column of Water, whoſe Height is 4 Times greater.

Thus, again, a Particle of Water running out, with 3 Degrees of Velocity, muſt move with 9 Times the Force of a Particle moving with but 1 Degree of Velocity; becauſe that Force is the Effect of a Cauſe 9 Times greater, *viz.* the Preſſure of a Column 9 Times higher:

Since no leſs than a Column 9 Times higher, is found, by Experience, neceſſary to make the ſeveral Particles of Water iſſue out with 3 Degrees of Velocity. So that, in theſe Two Inſtances, it ſeems to be certain, that the Forces communicated, are as the Squares of the Velocities. And that it is ſo univerſally, is argued thus: The Preſſures are as the Altitudes, and the Altitudes as the Squares of the Velocities of every ſingle Particle; therefore the Preſſures are as the Squares of the Velocities; but the Preſſures are the Cauſes of the Forces, with which the ſeveral Particles of Water iſſue out, or move; and therefore ſince Effects are proportional to their Cauſes, the Forces with which the ſeveral Particles iſſue out, and move, are as the Squares of the Velocities.

REMARK I. The Fault committed in this Reaſoning, and which quite runs through it, is the miſtaking a Part of the Effect for the Whole. The entire Effect of any of theſe Preſſures is, not barely a certain Number of Degrees of Velocity, in any ſingle Particle, but certain Degrees of Velocity in a certain Number of Particles, and that certain Number of Particles, in a given Time, is, confeſſedly, as the Degrees of Velocity.

REMARK II. And this leads me to a Second Remark, which is, That the entire Effect of theſe Preſſures being taken into Conſideration, ſeems to overturn this new Rule in Mechanicks for computing the Forces of moving Bodies, which is, *That the Forces are as the Quantities of Matter multiplied by the Squares of the Velocities.* And this I ſhall endeavour to make out thus: The Gentlemen who advance this new Rule, at the ſame Time that they aſſert the Velocities, in the Caſes of the Experiment above-mentioned, to be as the

Square

Square Roots of the Altitudes, do alſo confeſs, that the Quantities of the Fluid, preſſed out in equal Times, are as thoſe Velocities. For thus an ingenious Profeſſor abroad tells us in his *Epitome Element. Phyſ. Math.* Part. 2^da^. Cap. iv. p. 665. " *Quantitates fluidorum ex* " *utroque vaſe exeuntium in eodem tempore ſunt inter* " *ſe velut celeritates, adeoque in ſubduplicata ratione* " *altitudinum fluidorum ſupra foramina.* Now if this be true, that the Quantities of Water flowing out in equal Times, are as the Velocities, then the Forces cannot be as the Quantities of Matter multiplied by the Squares of the Velocities: For then the Effects, inſtead of being proportional, would be more than in Proportion to their Cauſes. Thus, the Effect of a Preſſure of a Column of any Fluid, as Water, 9 Inches high, inſtead of being but 9 times greater than that of 1 Inch above the Orifice, will be no leſs than 27 Times greater. For the Velocity being at this Height triple, the Quantity of Matter in a given Time will alſo be triple; which laſt, multiplied by the Square of the Velocity, gives 27 for the Force communicated by a Preſſure of 9 Inches in Altitude, while the Force communicated by the Preſſure of 1 Inch, is but as 1. So that the moving Forces produced will be as 27 to 1, while the Cauſes producing theſe Forces, are but as 9 to 1, *i. e.* Three Times too little for ſuch a Purpoſe.

Thus again, if the Velocities be as 1 and 4, the Quantities of Water iſſuing out will be as 1 and 4; but the Effects, or Forces produced, according to the new Rule, will be as 1 and 64; though the Preſſures, which communicate them, are but as the Altitudes, which are as 1 and 16. Whereas, to produce ſuch Effects, the Altitudes of the latter Column ought to have

been as 64; *i. e.* 4 Times greater than by Experience it is found to be.

REMARK III. I cannot but obſerve, in the laſt Place, that the common Rule of eſtimating the Forces of moving Bodies by the Quantities of Matter multiplied by their Velocities, is rather confirmed by theſe very Experiments. For then, according to the old Maxim, Effects are proportional to their Cauſes, the Forces communicated will be as the Forces communicating, or Preſſures. Thus let the Altitude, and conſequent Preſſure of any Column of Water be 9 Times greater than the Altitude of another; then the Velocity of every ſingle Particle of Water preſſed out will be triple, and the Number of Particles iſſuing out in a given Time will likewiſe be triple; therefore the Force reſulting from theſe Two multiplied together, according to the common Rule, will be 9, proportional to the Preſſure, as it ought to be. So again, if the Altitude be 16 Times greater, the Velocity will be quadruple, and the Number of the Particles quadruple, and the Force produced the Product of theſe Two; *i. e.* 16, ſtill proportional to the Altitude, or Preſſure.

And univerſally, the Forces communicated, according to the old Rule, are in a Ratio compounded of Two others, One of the Quantities of Matter, and the other of the Velocities: The Ratio of the Velocities, by the Experiments, is the Subduplicate Ratio of the Heights, and the Ratio of the Quantities of Matter is, by Confeſſion, likewiſe the Subduplicate of the Heights: Therefore the Compound of theſe 2 is the Ratio Integra, or ſimple Ratio of the Heights; in which Ratio are the Preſſures themſelves, which produce theſe moving Forces: So that, according to the common Rule, the Effects are always,

always, as they ought to be, proportional to their Cauſes.

After the ſame Manner the ingenious Mr. *'sGraveſande* once argued; for thus he reaſons in Paragraph 355 of *Phyſices Elem. Mathematica*, Firſt Edition.

" *Non tamen eandem cum profunditate proportio-*
" *nem ſequitur Velocitas, licet preſſio, ex quâ Veloci-*
" *tas oritur, in eâdem ratione cum profunditate*
" *creſcat. Quantitas motûs quæ in liquido produci-*
" *tur, eſt effectus totus preſſionis, & hæc quantitas*
" *ad inſtar preſſionis augetur. Ratio autem quanti-*
" *tatis motûs eſt compoſita ex ratione Velocitatis, &*
" *quantitatis materiæ motæ, hìc materia mota eſt a-*
" *qua, quæ ex foramine exit, cujus quantitas, ma-*
" *nente tempore, cum celeritate creſcit; dupla erit,*
" *ſi hæc dupla fuerit, in quo caſu motûs quantitas eſt*
" *quadrupla, id eſt, augetur ut quadratum celerita-*
" *tis, quod poſitâ celeritate quâcunque ſemper obti-*
" *net; creſcit ergo hoc quadratum ut preſſio, id eſt, ut*
" *altitudo liquidi ſuprà foramen ex quo profluit aqua.*

IV. *A Copy of a Letter from* Dr. David Kinneir, Coll. Med. Edingb. Soc. *to* Dr. Campbell, Coll. Med. Lond. Soc. Honorar. *touching the Efficacy of* Camphire *in* Maniacal *Diſorders.*

SIR,

IN Converſation, t'other Day, on the Subject of our Profeſſion, I told you how ſucceſsfully I had us'd Camphire in *Maniac* Diſorders, which hitherto never has

has fail'd my Expectation in the *Mania*, and other Caſes wherein the *Genus Nervoſum* and Blood ſeem to be affected after the ſame Manner, but in a leſs Degree. At your Requeſt, I ſend you the Hiſtory of Four Caſes, with all the Exactneſs my Memory can afford. I own I flatter my ſelf, that from the Effects of this Medicine Phyſicians may be led into a more ſure Way of Reaſoning in *Nervous Diſorders*, and therefore give you Leave to publiſh theſe Hiſtories, if you think they may be of Uſe to the Publick.

I. A *Gentlewoman of Nineteen Years of Age*, from an obſtinate Faſting for Two Days, and Averſion to ſee Company, in a religious Turn before *Eaſter*, fell into a deep Melancholy, would not talk, nor anſwer any Queſtion for ſome Time, but moan'd and ſigh'd continually; ſlept very little for Ten Days. This happen'd in the *Decline* of the Moon. The Night before the *Change* ſhe ſpoke, and call'd for ſome Water to drink, which being given her, ſhe immediately fell a ſtarting and laughing, and her Eyes got a Briskneſs in them ſomewhat uncommon, (as her Relations told me) then began to talk wildly, and continu'd ſo all that Night, She became next Morning very furious; whereupon a Phyſician was call'd, who bled her Four Times a Week the firſt Fourteen Days, vomited her, purg'd her, us'd the *Cold Bath*, and many other Methods common in ſuch Caſes, all to no Purpoſe. In this Condition ſhe continu'd for Nine Months; then I was apply'd to.

I firſt began her with an Antimonial Vomit, which had no other Effect than that of ſetting her faſt aſleep for Twelve Hours. Next Day I gave her half a Dram of *Camphire* in a *Bolus*, and as much at Night. She continu'd to reſt well all that Night, and had a great Moiſtneſs all

over

over her Body, and in the Day-time a plentiful Diſcharge by Urine. Thus for Four Days I ply'd her, and afterwards, in the Day-time, I order'd her Pills of *Æthiops, Gum-guaiac. Cinnab. Antim. & pulv. de Gutteta*; and at Night, the Doſe of *Camphire*. Senſible Alterations every Day for the better, and in Three Weeks Time ſhe enjoy'd the full Uſe of her Reaſon, and tho' Nine Years ago, at this Time is in a very good State of Health, having no extraordinary Ailing ever ſince.

II. A *Gentleman* of a good Family, *aged Seventeen Years*, (from ſome Diſappointment) became very ſilly and ſtupid. He continu'd in that Way for a long Time, notwithſtanding he had the beſt Advice. He had a great Inclination to chaw and eat every thing that came in his Way, be it what it would. He reſted pretty well a Nights, and was delighted with the Sight of every glaring Object. He hid every thing he could lay his Hands on, as well in Company as alone, and not any ways ill-natur'd. Thus for Half a Year he continu'd, when he became ſo furious, that Two Men could hardly hold him from beating every Body that came about him. Thus he would continue for Three or Four Days together, without ſleeping; then he would become calmer, after having ſlept ſome Time; but was a little miſchievous. This was the Way from *New Moon* till the *Full*; then he became ſilly and melancholly, ſpeaking little, and looking alway's down, not caring to look one in the Face. So it was *alternately* for Fourteen Months.

When I came to him, I found him full of Complaints of his ill Treatment; and had not I had a watchful Eye upon him, I had ſuffer'd from a Blow intended at me; but I got him to take the *Medicines* as *before*,

and

and with allowing him a more plentiful Diet, and a great Quantity of *Diluting Drinks*, he became perfectly well in Six Weeks, and has enjoy'd a good State of Health and ſound Judgment theſe ſeveral Years, and is now in Buſineſs.

III. A *Mercer*'s *Wife* of *Thirty Six Years of Age*, having born Four Children, and who was naturally of a lively, active Diſpoſition, fell ſo ill, all of a ſudden, One Day at Sermon, that with much ado they could get her out of Church with common Decency. She tore every thing about her, talk'd much, and utter'd horrid Oaths. In Fine, ſhe was as much diſorder'd as you can imagine any one. She had the beſt Advice, 'twas thought, that *Edinburgh* could give her. They fail'd of Succeſs, and left off viſiting her. About Half a Year thereafter I was conſulted about her. I treated her in the *foregoing* Manner, and in Four Days there was a ſenſible Alteration for the better, and in Four more ſhe went abroad. I ſtill continu'd the Uſe of the Medicines for Fourteen Days after Recovery, and now ſhe is as well as ever ſhe was in her Life.

IV. A *young Man*, *aged Twenty*, of a full Habit, was ſo very bad in the ſullen, deſpairing Way, wounding himſelf with his Teeth and Hands, that there was a Neceſſity of cloſe watching him. I *vomited* him twice, gave him ſome other Things common in ſuch Caſes; then began him with the *Camphire*, which in Ten Days brought him to Reaſon. He relaps'd after that on the *Change* of the Moon, was ill for Three Days, but not ſo as before. He came out of it again, and ſtood the *Change* of the next Moon with only a little Heavineſs, as in the *Hypo*. At the Height of the ſame Moon, the Day before, he was very uneaſy, and ſeem'd

to

to reſemble a Perſon greatly *hypochondriac*. I ſtill continu'd to him the *Camphire*, and the other *alterative Medicines*, for ſome Time, but in a much ſmaller Doſe than what I gave him in his Illneſs: So in Nine Weeks he was perfectly cur'd, and continues in good Health.

This, Sir, is the Subſtance of what I remember of the preceding Caſe, wherein I lay'd the whole Streſs of the Cure on *Camphire*.

I am, &c.

V. *A Method for determining the Geographical Longitude of Places, from the Appearance of the common Meteors, called* Falling Stars, *propoſed by* George Lynn, *Eſq; of* Southwick, Northampton-ſhire, *in a Letter to Dr.* Jurin, R. S. & Coll. Med. Soc.

SIR,

UPon peruſing, lately, the Account which the very ingenious Dr. *Halley* has given in the *Tranſactions*, N°. 360, of that extraordinary Meteor which appear'd all over *England*, 19th of *March*, 171⅓. I obſerve One very great Uſe he ſuggeſts might be made of thoſe momentaneous *Phænomena*, in determining the Geographical Longitude of Places, if we could but have the leaſt Notice of their appearing, *&c.*

I cannot but think, that ſome other Meteors which are very frequent, tho' little taken Notice of, might ſerve very well for the ſame Purpoſe. I mean thoſe which are vulgarly call'd *Stars ſhooting*, or *falling*, being a Sort of natural *Sky-rockets* diſcharg'd at a very great

Height, as I cannot but imagine from this Circumſtance, that they never appear, any of them, according to the beſt of my Obſervation, where the Sky is cloudy; and therefore, in all Probability, their Exploſion is in the Regions far above the Clouds, and they themſelves of the ſame Nature with (tho' perhaps leſs, and much lower than) that great Meteor above-mention'd, whoſe Height Dr. *Halley* computes to have been above 60 Geographical Miles, *viz.* much above the ('till then) reputed Limits of our Atmoſphere. But ſuppoſing theſe I mention to be diſcharged only at 20 or 30 Miles high, they may be ſeen by different Obſervers at the ſame Moment of abſolute Time, in very diſtant Places from one another, which is the Thing required: For, if in any Two Places, as the Doctor takes Notice, any Two Obſervers, by Help of Pendulum Clocks duly corrected by celeſtial Obſervations, do exactly note at what Hour, Minute, and Second, ſuch a Meteor is diſcharged, the Difference of thoſe Times will be the Difference of Longitude of the Two Places; nor does it require ſo much as the Uſe of a Teleſcope, as in the Methods hitherto put in Practice for that Purpoſe. Now theſe natural *Rockets* I have found to be very frequent in every Star-light Night; but eſpecially after a ſtormy Day, or in a ſtormy Night. If, therefore, Perſons who are prepared, as above, to be exact in their Time, and alſo have a moderate Knowledge of the ſeveral Conſtellations, ſo as to deſcribe the Track of any of thoſe Meteors amongſt the Stars, would but beſtow any determinate Hour to be agreed amongſt them, as for Inſtance, from 8 to 9 each ſuch Night, to watch and obſerve thoſe Exploſions, noting down immediately the Time and Track of them, it would be eaſy to determine,

termine, upon comparing their Obſervations, which of thoſe Exploſions each of them ſee at the ſame Time; and thereby the Difference in Longitude of thoſe Places would be exactly had, as above. It would, however, be worth the While, this Way, to try whether ſuch common Meteors are diſcharged, at any conſiderable Height above the Clouds, and how far, and whether they differ much from one another in their Heights.

But theſe Speculations I leave, Sir, to your better Judgment, either to improve the Hint, if it deſerves it, or if not, entirely to ſuppreſs it; and in either Caſe remain

Yours, &c.

VI. *An Attempt made before the* Royal Society, *to ſhew how Damps, or foul Air, may be drawn out of any Sort of* Mines, &c. *by an* Engine *contriv'd by the Reverend* J. T. Deſaguliers, L. L. D. *and* F. R. S.

THE *Engine* repreſented by the *Model*, conſiſts of a Triple *Crank* working 3 *Pumps*, which both ſuck and force Air, by Means of 3 *Regulators*, and are alternately apply'd to drive Air into, or draw it from any Place aſſign'd, thro' ſquare wooden Trunks; which being made of ſlit Deal, and 10 Inches wide in the Inſide, are eaſily portable, and joyn'd to one another without any Trouble.

EXPERIMENT I. I fill'd a tall cylindrick Glaſs with the Steams of a burning Candle and burning Brimſtone Matches, in ſuch Manner that a lighted Candle

would go out almoſt as ſoon as it was let down into that foul Air. Then fixing the Trunks (or ſquare Pipes) to the forcing Hole of the Engine, I drove freſh Air into the Bottom of the above-mention'd Receiver; ſo that the foul Steam came out at the Top of the Receiver, which was open.

EXPERIMENT II. Having fill'd another Receiver (cloſe at Top) with foul Steams, as before, I plac'd it in a Poſition almoſt horrizontal, only with the cloſe End ſomething above the open End, that the foul Steam might not go out of it ſelf, when ſpecifically lighter than common Air. I fix'd the Trunks to the Sucking-hole of the Engine; and by working the Engine, drew out the foul Steams from every Part of the Receiver, as the Trunks were applied to them ſucceſſively.

EXPERIMENT III. Having fill'd with foul Steams, and ſet upright (as in the firſt Experiment) the cylindrick open Receiver, I applied the Trunks to the ſucking Part of the Engine, with their open End near the Bottom of the Receiver. Then, by pumping, the Steams were all drawn downwards, and ſo out at the Top of the Trunks at the Engine; whereas, in the firſt Experiment, they were driven out at the Top of the Receiver.

EXPERIMENT IV. Having ſet a Candle in the cylindric Receiver above-mention'd, without having fill'd it with Steams, and let down the Trunks into the Receiver, below the Flame of the Candle, I laid the wet Leather over the Mouth of the Receiver, leaving about Half an Inch open, for the Air to come in; notwithſtanding which the Candle began to dwindle, and be ready to go out; but working the Engine with the

Trunks

Trunks joyn'd to the forcing Part, the Candle reviv'd, and burn'd, at laſt, as well as in the open Air. When I had left off Pumping, the Flame of the Candle diminiſh'd again; but when it was ready to go out, it reviv'd again, upon forcing in more Air with the Engine.

REMARKS *upon the* EXPERIMENTS.

WHEN Damps in Mines are ſpecifically lighter than common Air, they will be driven out of the Mine by the firſt Experiment.

When Damps are ſpecifically heavier than common Air, they may be ſuck'd out by the Second or Third Experiment.

When a *Sough*, or *Adit*, is carried from a Mine to any diſtant Valley, to diſcharge the Water, or ſave the Trouble of raiſing it quite to the Top of the Pit, *Shafts*, or perpendicular Pits are generally ſunk from the Surface of the Earth to the ſaid Sough, to prevent the Workmen from being ſuffocated as they dig the Sough, and that at a great Expence; but, by the 4th Experiment, freſh Air may be driven down to the Workmen, to continue their breathing free and ſafe, and to keep in their Candles; by which Means the Expence of perpendicular Shafts will be ſav'd.

It has been found by ſeveral Experiments, that a Man may breath a Gallon of Air in One Minute, and a Candle of Six in the Pound will burn nearly as long in the ſame Quantity of Air; therefore the Model only is capable of ſupplying freſh Air to One Man; and conſequently, a large Engine will abundantly ſupply Air for the burning of Candles, and the Working of a great Number of Men in a Mine.

One Man may work an Engine like the Model, and bigger every way in the Proportion of a Foot to an Inch.

As at every Stroke, 14 *cylindrick* (or 11 *cubic*) Feet of Air are driven in, or as many cubic Feet of Damp ſuck'd out, if the Axis of the Cranks be turn'd round 60 Times in a Minute, one Man, in that Time, may change the whole Air in a cubic Space, whoſe Side is 8 Feet; and One Horſe, by working 24 Pumps with Half the Velocity, will eaſily do 4 Times the Work of One Man.

The Engines work with a great deal of Eaſe, becauſe no Preſſure of Atmoſphere is to be remov'd; only a Velocity to be given to one Sort of Air, to change it for another.

Fire will not do in all Caſes, tho' in ſome, it will draw foul Air out of Mines with Succeſs; becauſe ſeveral Sorts of Damps extinguiſh Fire, and ſome fulminate, and are dangerous, when Fire comes near them; and even in common ſtagnant Air, Fire will not keep in long.

I am ſenſible, that large *Bellows* have ſometimes been made Uſe of for this Purpoſe; but they require a much greater Power to produce the ſame Effect, and cannot have the Advantage of being immediately chang'd from *Forcing* to *Sucking*; neither are they ſo cheap as the propos'd Engine, which may be all made of Wood, except the *Crank*, which muſt be of Iron, and the *Barrels* of very thin Copper.

VII. *A Letter to* James Theobald, *Esq*; *giving an Account of the* Norwegian Finns, *or* Finlanders, *by* Peter Kinck, *Esq*;

SIR,

INclos'd I send you an Account, the best I can collect, both from Persons of Veracity, as well as my own Observations, made in several Years Residence in the Neighbourhood of the People; which if it will be any Entertainment to you to peruse, will sufficiently answer the Pains of transcribing.

In the Kingdom of *Norway*, in those Confines which border upon *Sweden*, live a Sort of People call'd *Finns*, whose Habitation is in the Woods and Forrests, and who are some of them under the *Danish*, some under the *Swedish* Jurisdiction, of whose Original, Nature, and Manner of Life, I shall give some Account. And

I. Of their *Original*, which was from *Swedish Finland* Eastward of the Sea of *Bothnia*, from whence by Hunger, Times of Famine, and Want of the Necessaries of Life, they formerly transported themselves into *Sweden* and *Norway*, where in the Forrests and Wildernesses they got Leave to build and inhabit. For the common People of *Finland* are strong and hardy, and by Consequence fruitful and long-liv'd. *Finland* formerly was not so well cultivated as at present it is; so that the Produce of the Earth was not sufficient to subsist the Numbers of People born there; insomuch that Abundance of them (as the *Goths* and *Vandals*) were

were oblig'd to ſeek out new Quarters: And tho' theſe People have for the moſt Part kept to their native Language, yet have they made ſeveral Alterations in their Manners and Ways of living.

As for their *Language*, (tho' the much greater Part of them both underſtand and ſpeak the *Norwegian* Tongue, as well as the *Swediſh*) yet they moſtly uſe their own, which has not the leaſt Affinity or Reſemblance of either the other Two. As for Example, where the *Swedes* and *Danes*, in numbering tell 1, 2, 3. *Een, Toe, Tree,* they in theirs tell *Tx, Kax, Kolime*; and as the Firſt ſay, for Give me Bread, *Giv. mig brod,* they ſay *Alla mina leip*, &c. And when we ſtand and hear them converſe together, we are ſurpriz'd to conceive how they underſtand each other, ſince they ſpeak ſo very low, that we can ſcarce hear them.

II. Of their *Nature*. They are generally low of Stature, but ſtrong and hardy, and healthy; their Eyes are lively, their Noſes high, and their Teeth even and white, their Feet ſhort. The Women are generally ſo ſtrong of Conſtitution, that in Child-bearing they ſeldom need any Aſſiſtance, and ſoon return to the Buſineſs of the Family again, except here and there a weakly Conſtitution. They are generally ignorant and ſilly; but this muſt be attributed to the little Converſe they have with each other and the reſt of the World, ſince the *Men*, whoſe Affairs call them often to traffick with their Neighbours, and in different Provinces, are Men of good natural Parts, ſharp, and who look well to their Intereſt. They are frugal, parſimonious and humble, fearful of giving Offence, and very reſpectful to their Superiors: They will work whole Days without any Food, if they can only have Tobacco to ſmoak or chew.

III. Of

III. Of *their Manner of Life.* And this is a Subject that I could be very copious upon; but that I mayn't be tedious, what follows ſhall ſuffice. Their Food conſiſts chiefly in a Sort of Fiſh which they call *Oret*, and which anſwers to our Salmon-Trout; ('tis a very delicious fat Fiſh, which they catch in the Rivers that run thro' the Woods) and next to that, in Bread and Flour of *Rye*, which they ſometimes get great Plenty of, by cutting down and burning whole Forreſts, and ſowing the Ground with Rye, which ſometimes produces 30 or 40 Fold. But this Method of producing has been of late very ſtrictly forbidden, under very ſevere Penalties, by the Landlords of thoſe Forreſts, by Reaſon of the immenſe Dammage it does, by the Loſs of ſo much Timber; and becauſe the Fire ſometimes lays waſt immenſe Tracts of Land: So that it is with great Difficulty extinguiſh'd, burning whole Months together, to the great Detriment of Trade.

They frequently uſe ***Bathing***, at leaſt once a Month, thinking thereby they prevent Sickneſs, and diſſipate all Weakneſſes from the Body: And their Method of Bathing ſeems ſo very particular, as would ſcarce agree with any other Conſtitution, or meet with Approbation from Phyſicians. The Method is this: In the Middle of the Houſe (which generally conſiſts of One large Room, built all of whole Timbers laid croſs, and notch'd in at the Ends to let them cloſe, and then caulk'd with *Moſs*, as the Seams of a Ship are with *Okam*) they build an Oven with Stone without Mortar, and without a Funnel; the Smoak going out at a Hole in the Roof, which is left open while the Wood is burning in the Oven, but ſhut cloſe as ſoon as 'tis all burnt to a bright Coal, which keeps in all the Heat. When the Oven is thus

made red hot, they then ſtrip, both Men and Women, without any Reſerve, and place themſelves on Benches made near the Roof on Purpoſe: Then cold Water is brought in, which from Time to Time is ſprinkled upon the Oven; from whence ariſes a thick Steam on the Bathers, which makes their Bodies ſo warm, that they ſweat very plentifully. Each Perſon has a Rod in their Hand, with which they gently beat their whole Bodies; and if they find themſelves ſo hot that they cannot well endure it, they call for cold Water, which they pour over themſelves in ſo dextrous a Manner, that it diffuſes its ſelf over their whole Bodies, and ſo cools them again. Thus when they have bathed ſufficiently, they go directly out into the Air, tho' in the moſt inclement Seaſon of the Year; and what is more, will roll themſelves in the Snow for a good while together, and get no Harm by ſo doing: And this Method of Bathing they make Uſe of as their ordinary Cure, when they find any Indiſpoſition of Body upon them.

In the Winter, when the Ground is cover'd with Snow, they make uſe of a Sort of long wooden Shoe, *Inducunt ſoleas ligneas tres vel quatuor ulnas longas*, on which they go ſo ſwiftly, that in 2 Hours Time they will run 13 or 14 Miles: And as they are generally good Marks-men, with their Guns they kill Abundance of wild Game, both to help to ſupport their Families, and ſell to buy themſelves Neceſſaries.

They are very ignorant of Chriſtianity, by Reaſon of their great Diſtance from any Towns; but that Misfortune the preſent King of *Denmark* has taken ſome Meaſures to remedy: Yet tho' they ſeem ſo ignorant and barbarous, yet 'tis very rare that any of them are guilty of any conſiderable Crime.

I am, &c.

VIII. *A preternatural Perforation found in the upper Part of the* Stomach, *with the* Symptoms *it produc'd, by Mr.* Chriſtopher Rawlinſon, *Surgeon.*

James Skidmore had complain'd for Three or Four Years laſt paſt, of a violent Pain in his Stomach and Bowels, never being able to reſt in his Bed at Night, 'till he had vomited up the greateſt Part of what he had eat or drank the Day before. He would often compare his Pain to ſome great Weight laying upon the Region of the Stomach, which he in ſome Meaſure alleviated, by preſſing hard with his Hand upon that Part. When he turn'd himſelf in Bed from one Side to the other, he told me, he could plainly perceive that ſome Fluid or other fell down with Noiſe to the depending Side; which Fluid he believ'd to be the Occaſion of all his Miſery: For which Reaſon he often ſaid, he would willingly conſent, nay, often earneſtly preſs'd that the Surgeons would cut him open (as he expreſſed it) and let it out.

He had no apparent *Tumour* upon the Part, nor was his Belly more *extended* than uſual. He had had the Advice of ſeveral able Phyſicians before he came into the Hoſpital, but all without the leaſt Amendment to his Diſeaſe. When he died we were deſired to open him, and try if we could find out the Cauſe of his Complaints. As ſoon as we had penetrated the *Peritonæum*, there flow'd out a whitiſh Liquor, not much unlike to *Whey*, only a little more thick and fæculent;

nor did it emit ſo noiſom a Smell as might be expected from its long Reſidence in that Place. We computed there were above Four Quarts of this Liquor contain'd in the *Cavity* of the *Abdomen.*

When we came at the *Stomach* we found it *perforated* in its *upper* Part, about the middle Space betwixt the Two Orifices, wide enough to contain the End of one's Finger. We cut it open Length-ways, and found it pretty full of a thick glutinous Matter, inclining to be yellow; and to its inner Coat, on the lower Side, there firmly adher'd the Stone of a *Prune*, or ſome other Fruit reſembling it. On its Inſide, near the preternatural Perforation, 'twas gangreen'd for two or three Inches; and on the other Side of the Perforation there was an *Ulcer* near the ſame Bigneſs. The whole Stomach was a great deal thicker than uſual; but that Part next the *Pylorus* was above Four Times thicker than in a natural State. It *adher'd* cloſely to all the Parts about it; and to the *Pancreas* 'twas ſo firmly ty'd down, that it could not be ſeparated without tearing. The Spleen did not exceed a Quarter of an Ounce in Weight. The *Pancreas* was *ſchirrous*, tho' pretty near its natural Size. In the *Liver* and *Kidneys* there was no apparent Defect; nor had the Parts in the *Thorax* receiv'd any viſible Alteration, except that the *Lungs* adher'd more firmly to the *Pleura* than uſual. The *Inteſtines* and all the *Viſcera* contain'd in the *Abdomen* were of a whiter Colour than uſual, by being ſo long ſodden in the Liquor they floated in.

IX. *An Account of an Humane Skeleton of an extraordinary Size, found in a Repoſitory at* Repton *in* Derby-ſhire, *together with ſome Examples of long Life, communicated by* Simon Degg, M. D. & F. R. S.

Extract from my Journal, Friday July *the* 15*th*, 1726.

THE Day favouring us, we ſet forward from *Derby* for *Repton*, *alias Repingdon*, and in One Hour and Two Thirds we arrived at *Repton*, a Town in the *Trent* famous for the Burial of the *Mercian* Kings, whoſe chief Seat was at *Tamworth* in *Stafford-ſhire*. This Town is likewiſe remarkable for its Free-ſchool, and its antient Abbey. Having view'd the Ruins in this Town, and enquiring for Antiquities, the Inhabitants brought us *Thomas Walker*, a Labourer, aged Eighty Eight Years, who gave the following Account.

About Forty Years ſince cutting Hillocks, near the Surface he met with an old Stone Wall, when clearing farther he found it to be a ſquare Encloſure of Fifteen Foot: It had been covered, but the Top was decayed and fallen in, being only ſupported by wooden Joyces. In this he found a Stone Coffin, and with Difficulty removing the Cover, ſaw a *Skeleton* of a *Humane Body Nine Foot* long, and round it lay *One Hundred* Humane Skeletons, with their Feet pointing to the Stone Coffin. They ſeem'd to be of the ordinary Size. The Head of the great Skeleton he gave to Mr. *Bowers*, Maſter of the Free-ſchool. I enquired of his Son, One

of

of the preſent Maſters, concerning it, but it is loſt; yet he ſays, that he remembers the Skull in his Father's Cloſet, and that he had often heard his Father mention this Gigantick Corps, and thinks this Skull was in Proportion to a Body of that Stature. The Bottom of this Dormitory was pav'd with broad flat Stones, and in the Wall was a Door-caſe, with Steps to go down to it, whoſe Entrance was Forty Yards off, nearer the Church and River. The Steps are Stone, and much worn: 'Tis in a Cloſe, on the North Side of the Church, and over this Repoſitory grows a Sycamore, planted by the old Man when he filled in the Earth. The preſent Owner will not ſuffer it to be open'd, the Lady of the Mannor having forbidden it. This was atteſted to us by ſeveral old People, who had likewiſe ſeen and meaſured the Skeleton.

EXAMPLES *of Long Life.*

AT *Uttoxeter*, in the Moorlands of *Stafford-ſhire*, were buried the following Number of People at the following Ages, in the Year 1702.

In 14 Days were buried Three Women, the Firſt 103, the Second 126, and One of 87; and the ſame Year Two Siſters and a Brother in and of that Pariſh, the Brother 92, the youngeſt Siſter 95, the eldeſt Siſter 98.

And in the ſame Pariſh this preſent Year 1726, were buried in 22 Days, the following aged People.

Auguſt the 7th One Woman aged 94, and One Man aged 81.

Auguſt the 4th One Man, aged - - - - - 68

Auguſt the 19th One Man, aged - - - - - 87

Auguſt

Auguſt the 22d One Man, aged - - - - 82
Auguſt the 23d One Man, aged - - - - 83

Men 4
Women 2

In 1726. at *Gravely* in *Hertford-ſhire*, 31 Miles from *London*, almoſt every Sixth Soul is upwards of Sixty. Inhabitants 187. aged above Sixty, 29.

X. *A farther Account of the Bills of Mortality, &c. of ſeveral conſiderable Towns in* Europe, *for the Years* 1722 *and* 1723, *extracted from the* Acta Breſlavienſia, *by Sir* Conrad Spengell, R. S. S. & Col. Reg. Medic. Lond. Lic.

A Liſt of thoſe that were born and buried, &c. in Breſlaw, &c. *in the Year* 1722.

		Chriſtened	
From the 25th of *Dec.* to the 31ſt, 1721.	*Buried* 35	Males - - -	665
January, 1722. - -	100	Females - - -	670
February, - - -	94		1335
March, - - - - - - -	129	*Married* 424 *Pair.*	
April, - - - - - - -	121	Among the Dead were	
May, - - - - - - -	122	Married Men - -	231
June, - - - - - - -	182	Married Women -	149
July, - - - - - -	185	Widows and Widowers	150
Auguſt, - - - - -	203	Batchelors - -	57
September, - - - -	167	Maidens - - -	52
October, - - - - -	173	Children to 10 Years of Age: Boys	570
November, - - - -	190	Children to 10 Years of Age: Girls	499
December, 'till the 24th	90	Still-born: Boys	53
	1791	Still-born: Girls	30
			1791

In

In *Vienna*
Buried,

Men,	1038
Women,	942
Boys,	1551
Girls,	1430
	4961

Among which was, 1 of 105, 1 of 110, and another of 111 Years of Age.

Chriſtened
Children, - - - - - - - 4417

In *Dreſden*,
Buried 1519, *Chriſtened* 1514, *Married* 451 Pair.

In *Leipſig*,

	Buried								Chry-ſoms	Still-born					
	Married Men	Married Women	Batchelors	Maidens	Boys	Girls	Child-bed-Women	Males	Females	Males	Females	Widowers and Widows	*Summa*
January	13	6	9	4	20	23	3	4	0	5	3	11	101
February	17	11	11	8	14	7	0	3	3	4	2	13	93
March	10	6	3	4	21	11	3	9	1	6	2	8	84
April	19	9	4	10	17	8	1	3	3	1	2	15	92
May	19	13	13	2	20	14	2	5	2	2	4	15	111
June	11	7	4	2	22	26	0	4	2	5	5	1	89
July	11	8	4	2	24	22	3	4	3	1	2	3	87
Auguſt	8	5	6	8	16	9	1	4	3	2	4	2	68
September	8	3	1	1	19	14	1	3	2	1	2	6	61
October	6	9	4	5	12	8	1	6	1	3	6	4	65
November	11	7	3	3	10	14	1	7	0	1	2	5	64
December	11	7	4	4	19	13	0	6	3	4	2	6	79
Summa	144	91	66	53	214	169	16	58	23	35	36	89	994

Chriſtened

Chriſtened

	Males	Females	Total	Pairs Married
January	44	21	65	25
February	30	26	56	21
March	36	31	67	29
April	39	28	67	0
May	39	41	80	53
June	40	35	75	23
July	40	39	79	55
Auguſt	40	34	74	27
September	34	43	77	22
October	49	30	79	24
November	33	38	71	76
December	39	39	78	6
Summa	463	405	868	361

So that in this Year there died 994, and Chriſtened 868. Hence there are 126 fewer born than died.

Among the Chriſtened were
2 Poſthumous,
12 Twins,
99 Baſtards,
1 Foundling.

Among the Dead were

78 from 60 to 69 Years old,
50 70 79
15 80 98

In Weimar,

Buried 143, *Chriſtened* 190. Among the latter were 107 Boys and 83 Girls, and among theſe were 6 Twins and 1 Baſtard. 73 Pair were married.

In Berlin,

Buried 2499, *Chriſtened* 2701. Among which were 260 Baſtards. *Married* 742 Pair. So that there were 202 more *born* than *buried.*

The Special LIST *of the Kingdom of* Pruſſia.

	Chriſtened	Pairs Married	Buried
IN *Angerberg* - - - -	565	92	212
Balga - - - - - -	604	145	400
Bartenſtein - - -	268	42	124
Barthen - - -	309	97	170
Brandenburgh - -	937	223	546
Dutch Elau - -	157	20	54
Fiſhauſen - -	473	105	405
Gerdauen - -	545	92	187
Gilgenburg - -	205	63	98
Inſterburg - -	2047	381	1013
Johannisburg - -	382	68	191
Labiau - -	608	99	280
Liebſtadt - -	443	76	179
Lotzen - -	263	57	110
Lyck - -	360	93	119
Marienwerder - -	791	185	398
Mummel - -	913	225	521
Neydenburg - -	674	153	301
Neuhauſen - -	217	62	141
Neuhoff - -	37	2	25
Oletzko - -	726	123	270
Ortelsburg - -	501	91	260
Oſterode - -	506	97	207

In

		Christened	Pairs Married	Buried
In *Preuſs Elau* - -		490	96	276
------- *Holland* - -		666	147	334
------- *Marck* - -		142	170	287
Ragnit - -		803	109	371
Raſtenburg - -		553	118	301
Rein - -		551	81	174
Schacken - -		524	130	390
Schonberg - -		279	66	114
Seheſten - -		356	76	137
Tapiau - -		846	176	401
Tilſit - -		1267	218	632
The Cities of *Koningsberg*		1664	442	1688
	Total	20672	4420	11316
In *Cuhrmarck Brandenburg*		18334	4732	11602
Neumarck - -		7509	1705	4047
The Dukedom of *Magdeburg*		8402	2264	6006
of *Cleve* and *Mark*		6921	1886	5740
Pomerania		7256	1858	4424
Halberſtadt		2899	724	2040
Hohenſtein		589	151	403
Minden		2140	479	1351
Ravensberg		2270	605	1600
Mors		491	160	369
Geldern		1686	447	1449
Tecklenburgh		579	118	398
Lingen		729	242	529
Lauenberg and *Butow* -		630	156	372
French Colonies - -		663	130	572
Of all the King of *Pruſſia*'s Dominions	Summa	81770	20077	52218

So that there were 29552 more *Chriſtened* than *Buried.* Among the *Dead* were 71 above 90 and 100 Years of Age; and One of 120.

In *Nurenberg,*

Buried,		*Born,*	
Married Men, -	194	Males, - - -	501
Women,	233	Females, - -	554
Batchelors, - -	42	*Summa*	1055
Maidens, - -	61		
Boys, - -	285	Among which were	
Girls, - -	230	12 Twins.	
Still-born, -	12		
Summa	1057		

In *Copenhagen,*

Born,		*Married,*	*Buried,*
Males,	1345	785 Pair.	1999
Females,	1256		
	2601		

So that there were 602 more born than buried.

In *Amſterdam Died* 8421.

In *Epperies* were *Born*	213	*Buried*	135			
In *Dantzig* were *Born*	2092	*Married*	490 Pair.	*Buried*	1442	
In *Rawitz* were *Born*	200	*Buried*	74			

The

The Bills of Mortality of the Year 1723. *from the* Acta Breſlavienſia.

In *Breſlaw*,

From the 25th of *Dec.* to the 31ſt, 1722.	*Buried* 22	*Chriſtened*	
January, 1723.	118	Males	711
February,	114	Females	684
March,	122		1395
April,	102	*Married* 422 *Pair.*	
May,	115	Among the Dead were	
June,	119	Married Men	220
July,	93	Married Women	118
Auguſt,	124	Widows and Widowers	150
September,	124	Batchelors	48
October,	85	Maidens	46
November,	114	Children to 10 Years of Age: Boys	369
December, 'till the 24th	69	Girls	306
	1321	Still-born: Boys	38
		Girls	26
			1321

In *Vienna*,

Buried 5443. Among which were 1079 Men and 974 Women, 1758 Boys, and 1632 Girls. *Chriſtened* 4457.

In *Lobau*,

Buried 171. *Chriſtened* 226, and 70 Pair *Married*.

In

In *Freyberg*,

Buried 321 *Chriſtened* 362

In *Dreſden*,

Buried 1654. That is, 165 Married Men, 136 Married Women, 36 Widowers, and 138 Widows, 68 Batchelors, 64 Maidens, 580 Boys, and 467 Girls.

Still-born, Boys 54 and Girls 46. *Chriſtened* 1510. That is, 756 Boys and 754 Girls. Among which were *Baſtards* 66 Boys and 41 Girls. *Married* were 396 Pair.

In *Leipſig*,

Buried 928. Among which were 126 Married Men and 88 Married Women, 53 Batchelors and 36 Maidens, 199 Boys and 164 Girls, 21 Women in Childbed, New-born Boys 60, and Girls 46, beſides Still-born Boys 37, and Girls 30; alſo 68 Widows and Widowers. *Chriſtened* were 966; as 489 Boys and 477 Girls Among which were 11 Pair Twins and 99 Baſtards. *Married* have been 306 Pair.

In *Erfurt*,

Buried 448, *Chriſtened* 666.

In the Elector of *Mentz*'s Dominion, in 75 Villages are *Buried* 543, *Chriſtened* 872.

In *Berlin*,

Buried 2618, *Chriſtened* 2770. Among which are 289 Baſtards.

In

In the Towns and Villages of the *Old*, *Middle*, and *Uckermark* are *Chriſtened* 19058. Among which are 995 Baſtards. There were *Married* 4943 Pair, and *Buried* 13317. Among which were 23 who lived to 90, 100, and One to 112 Years old. So that there were 5741 more Born than Buried.

In the reſt of the King of *Pruſſia*'s Dominions.

	Born	*Married*	*Buried*
In the Kingdom of *Pruſſia*,	21384	4957	10762
In the Elect. of *Brandenburgh*,	18774	4863	13015
In *Neumark*, - -	6907	1813	4193
In the Dutchy of *Magdeburg* and County of			
Mansfeld, - - - -	8858	1781	5579
Cleve and *Mark*, - -	6645	1924	6848
Pommern, - - - -	8544	2241	5294
Halberſtad, - - -	2943	808	2162
Hohenſtein, - - -	604	165	389
Minden, - - -	2087	565	1925
Ravensberg, - -	2061	613	1833
Moers, - - -	484	156	371
Geldern, - - -	1866	436	1478
Tecklenburg, - -	475	141	437
Lingen, - - -	701	270	641
Lauenberg and *Butow*, -	588	149	340
French Colonies - -	594	177	563
	83515	21059	55830

Among the *Born* are 2157 Baſtards, and among the *Buried* 77 that were above 90 and 100 Years old. So that there were 27685 more Born than Buried.

In

In *Copenhagen* were *Born* 2604, *Buried* 1914, *Married* 701 Pair. Therefore 690 more Born than Buried.

In *Amsterdam* there *Died* 7119, and were *Married* 2289 Pair.

In *Epperies*, *Born* 203, *Died* 132.

In *Dantzig*, *Born* 2002, *Buried* 1495, and *Married* 537 Pair.

I have but just got the *Acta Breslaviensia*, or else would have sooner sent this.

Adieu, &c.

XI. *Ingens Materiæ Purulentæ copia cariem & separationem totius* Ossis Mali, *&c. inducens, & per triennium ex Ore Pueri defluens, observata à Domino* Petro Hardisway, M. D.

DIE Sexto Mensis Febr. 1724. advocabar ad Puerulum decennem, qui à sex septimanis febre lentâ decumbebat malè multatus. Præscriptis quæ res postulabat remediis, intrà sex vel septem dierum spatium à morbo liber evasit: Verùm diebus aliquot elapsis, ingens in Genæ dextræ interioribus subortus est Tumor, puero nihilominùs, quoad cætera benè valente. Ore ejus ægrè diducto, in conspectum veniebat ingens Puris quantitas, at unde manaret, præ Pueri protervià non licuit pro hâc vice explorare. Ideò consulebam tantùm, ut ficum in lacte coctam ad tumorem emolliendum, atq; si fieri posset rumpendum admoverent. Mane sequenti Os incredibili ferè Puris copiâ, teterrimi odoris (tumore intereà disrupto) diffluebat. Gargarismate modicè detergente præscripto, circà vesperum Pus pari omninò quantitate

Fig. 1.

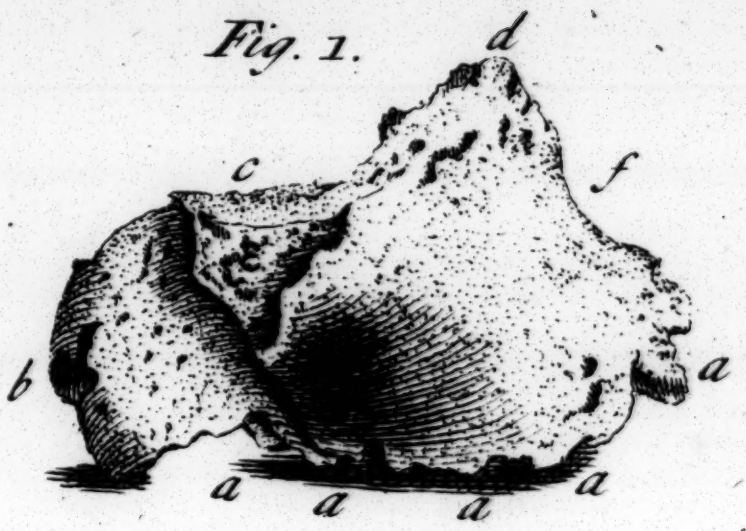


Fig. 2.

Fig. 3.

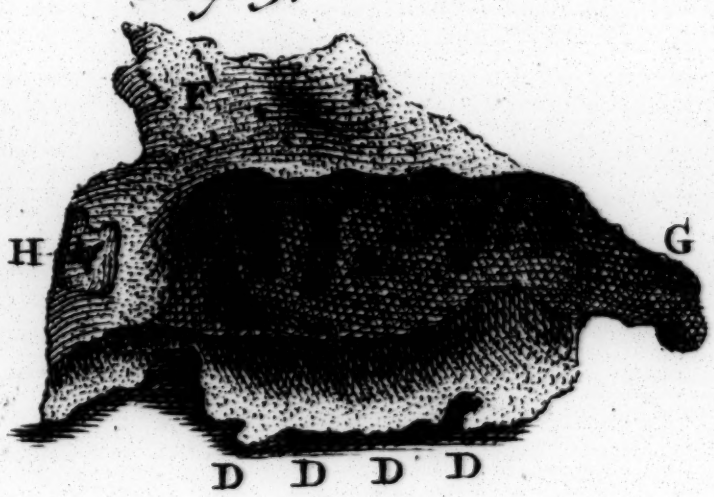
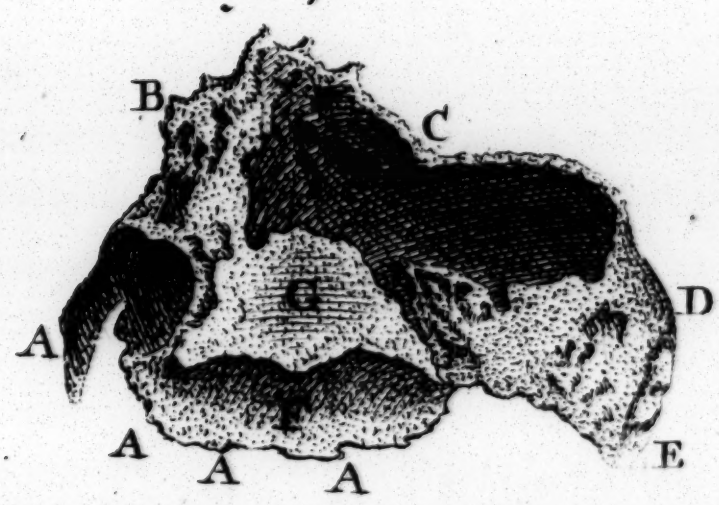


Fig. 4.



quantitate atq; priùs depluebat. Mirabar ſanè unde hoc provenire poſſet, cùm Ulcus in Genâ tanto proventui nequaquàm ſufficere videretur; ſuſpicabar autem (fœtore præ cæteris ſuadente) materiæ iſtius fontem in capite alicubi intrà oſſa latitare: quod cùm paulò diligentius inveſtigarem, ecce *Os Maxillæ ſuperioris ad dextras* carne (quæ quaſi ſemicocta utrinq; receſſerat) denudatum prorſùs atq; *Carie* correptum, & ex *Dentium Alveolis* (dentes enim puerulus, ceu inutiles, ſuis nullo negotio jam extraxerat digitis) hoc materiæ purulentæ profluvium jugiter delabi: quapropter Chirurgus è viciniâ advocatus eſt, cui committebatur negotium. Huic ſuadebam, ut dum *Oſſis* medelæ incumberet, *Ulceris* itidem deterſioni & conſolidationi operam daret; verùm quicquid dicerem, nulla quæcunque Genæ ſuppuratæ adhibere voluit medicamina: quo factum eſt, ut ea iterum in molem ingentem, atq; alterâ (ſiniſtrâ ſc.) duplò major citò excreſceret. Mandibulæ Carioſæ ſolummodò penicillum tincturâ quâdam, forſan *Myrrhæ*, imbutum applicabat. At longo temporis ſpatio tandem emenſo, cùm parùm ſuâ tincturâ proficeret, hamulo admoto, quod temporiùs factum debuerat, *Os Mali* integrum evellebatur. Ab Oſſis extractione Gena nequicquàm ſubſidebat, ſed potiùs incrementum ſumebat, & materiæ purulentæ copia nullatenùs minuebatur. Oſſe autem abrupto, *Oſſis Palati* dimidium cum *Narium Septo* abſumptum eſt; parte etiam infimâ *Orbitæ oculi* ſimul abreptâ, ut in Iconibus adjectis clariùs oſtendere ſim conatus. His addendum, quod Tumore *Proceſſui Zygomatico* ſuperſtiti incumbente, *Gena* mole duplò inſignior *ſiniſtrâ* extat, & oculum pœnè claudit, pure quaſi ex fonte perenni indeſinenter fluente.

Fig. 1*ma.* Facies Oſſis externa.

a, a, a, a, a. *Alveoli* Dentium.

b. Dens *molaris* remotior.
c. Pars *Orbitam* oculi faciens.
d. Pars Oſſis ubi abruptum circà *Naſum.*
e. Pars Oſſis ubi *Proceſſus Zygomaticus* terminatur.
f. Pars Oſſis ſub *Labium ſuperius.*

Fig. 2*da.* Forma interior Oſſis.
1 Pars ad *Naſum* abrupta.
3 Pars *Mandibulæ* exterior & Dentis *alveolus.*
4 Pars Oſſis *oculum* attingens.
5 Pars *Maxillæ* remotior.
6 Pars *Naris dextræ, ſepto* abrupto.
7 Dens *Molaris.*

Fig. 3*tia.* Os reſupinatum exhibet.
D, D, D, D. Pars *Oſſis Palati.*
E, E, E. Alveoli dentium.
F, F. Pars *Mandibulæ exterior.*
G. Pars *anterior* ejuſdem.
H. Dens Molaris.

Fig. 4*ta.* Facies interior Oſſis parùm reclinati.
A, A, A, A. Pars *Mandibulæ,* & *Alveoli* Dentium.
B. Locus ubi os abruptum circà *Naſum.*
C. Pars oſſis *Oculum* attingens.
D. *Maxillæ* pars remotior.
E. *Dens Molaris* remotior.
F. Pars *Oſſis Palati.*
G. Pars *Naris dextræ ſepto* abrupto, ut in Fig. 2^da^ N° 6.

Poſt hæc, idem vir peritiſſimus per Literas die 24. Decemb. 1727. datas, *Editorem* certiorem fecit, "Tumorem ſuprà deſcriptum in eodem adhuc remanere ſtatu, "Materiâ Puriformi Os & Fauces prò ſolito irrigante. "Puer cætera ſanus edit, bibit, dormit, & unà cum "aliis ſuæ ætatis quotidiè alacriter ludit.

F I N I S.

ERRATA. No. 399. Pag. 294. lin. 11. lege *pyri-inciſis.* Ibid. l. 19. leg. *latifolia.* P. 303. l. 19. leg. *are.* P. 315. l. 23. leg. *Curienſis.* P. 317. l. 11. leg. *cuticula.* P. 324. l. 17. leg. *ſame.* Ibid. l. 26. leg. *animals.* —— No. 400. P. 351. l. 7. leg. *eaſes.* P. 354. l. 9. leg. *horizontal.*

Numb. 401.

PHILOSOPHICAL TRANSACTIONS

For the Months of *January*, *February*, and *March*, 1728.

The CONTENTS.

I. *The Anatomy of the* Poiſonous Apparatus *of a* Rattle-Snake, *made by the Direction of Sir* Hans Sloane, *Bart.* Præſ. Soc. Reg. & Coll. Med. *Together with an Account of the quick Effects of its Poiſon*; *by* John Ranby, *Eſq*; *Surgeon to his Majeſty's Houſhold, and F. R. S.*

II. *A Letter from the Rev. Dr.* Samuel Clarke *to Mr.* Benjamin Hoadly, *F. R. S. occaſion'd by the preſent Controverſy among Mathematicians, concerning the Proportion of* Velocity *and* Force *in* Bodies in Motion.

III. *Aſtronomical Obſervations made at* Vera Cruz, *by Mr.* Joſeph Harris. *Reviſed and communicated by* Edm. Halley, L L. D. Aſtron. Reg. & R. S. S.

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V. *Some Observations towards composing a natural History of* Mines *and* Metals, *communicated in a Letter to Dr.* Rutty, S. R. Secr. & Coll. Med. Lond. Soc. *By Dr.* Francis Nicholls, *Professor of Anatomy at* Oxford.

VI. *Observationes Astronomicæ habitæ* Ulyssipone, *Anno* 1726, *à Rev. P* Joh. Baptista Carbone. *Soc. Jes. Communicante* Isaaco Sequeyra Samuda, *M. D. R. S. S. & Coll. Med. Lond. Lic.*

VII. *Some Observations made in the Dissection of three Subjects; by Mr.* Ranby, *F. R. S.*

Jos: Banks

Plate 1.

Philo: Trans:

Fig. 1.

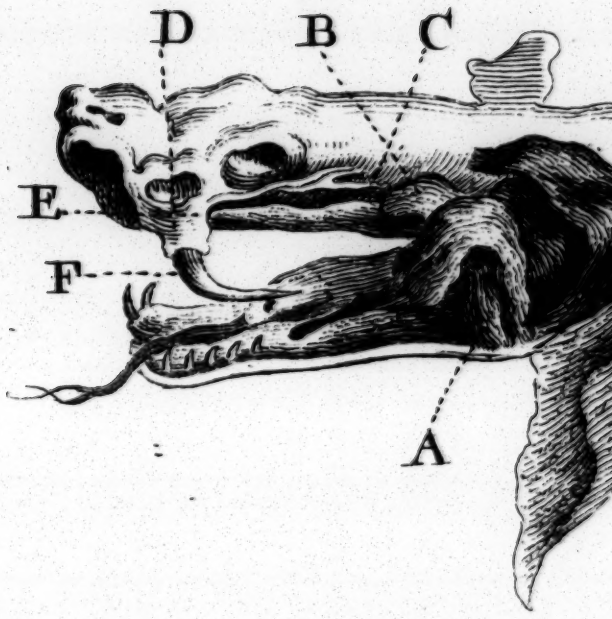


Fig. 2.

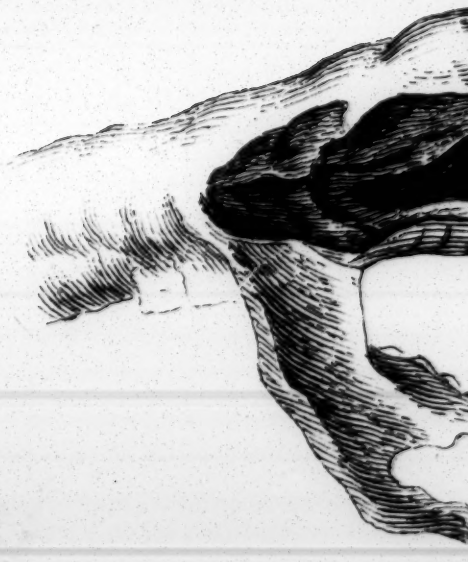


Fig. 3.

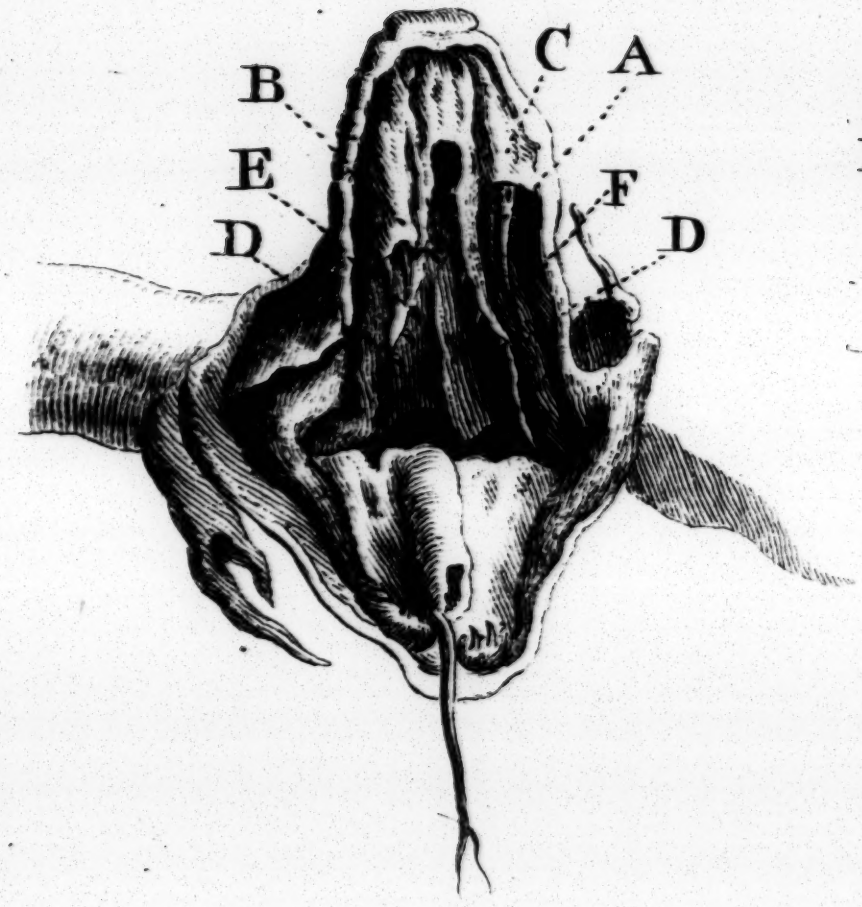
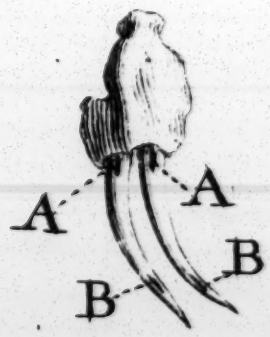


Fig. 4.



2.

Plate 1.

Fig. 3.

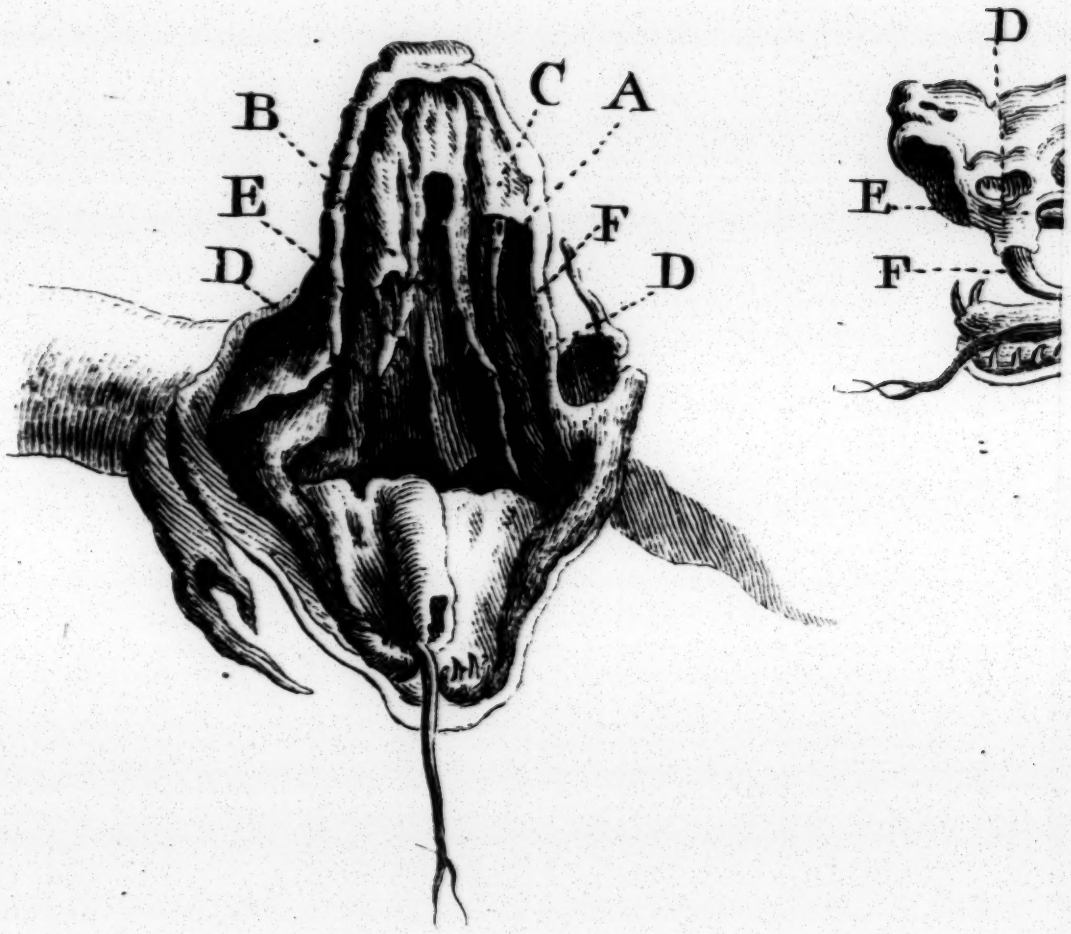
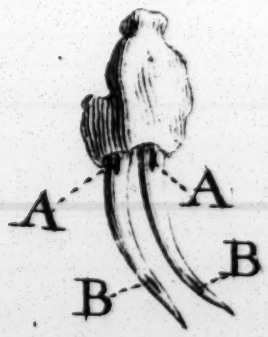


Fig. 4.



I. *The Anatomy of the* Poiſonous Apparatus *of a* Rattle-Snake, *made by the Direction of Sir* Hans Sloane, *Bart.* Præſ. Soc. Reg. & Coll. Med. *Together with an Account of the quick Effects of its Poiſon*; *by* John Ranby, *Eſq*; *Surgeon to his Majeſty's Houſhold, and F. R. S.*

THIS Animal was ſent from *Virginia* to an eminent Merchant, and having been by him preſented to Sir *Hans Sloane*, it was plac'd in my Hands, on Purpoſe to make ſuch Experiments with it as might inform Mankind of the fatal Symptoms which attend its Bite, and the Appearances in the dead Bodies of ſuch Animals as have been bit by it. It is only by this Method, and a Number of Facts faithfully ſtated and compared with each other, that we may hope one Time or other to diſcover the Manner of the Poiſon's operating, and perhaps (a thing of the utmoſt Conſequence) to find out ſome Remedies, internal or external, to relieve Perſons bit by it. The Anatomy of the Rattle-Snake having been ſo accurately deſcribed by the late ingenious Dr. *Tyſon*, very little more can be added to his Account; I ſhall therefore only take Notice of the *Inſtruments* of its Poiſon, ſome of which are different from what that celebrated Anatomiſt obſerv'd. Removing then the common Integu-

ments of the Head, the *Muſcles* that raiſe the poiſonous Fangs appear; the firſt of which ariſes with a ſhort fleſhy Beginning from the upper Edge of the lower Jaw, near the Articulation of one of thoſe Bones which Dr. *Tyſon* calls *Maxillarum Dilatores* (See *Fig.* 1. A.) and ſends a few carnous Fibres to the Side of the *Cranium*; then becomes tendinous, and ſo marches to its Inſertion in the Outſide of the Bone which receives the poiſonous Fang. (See *Fig.* 2.) Diſplacing this Muſcle there appeared a *Gland*, (See *Fig.* 1. B.) about the Bigneſs of a ſmall Pea, which I take to be one of the *Maxillary* Glands, for the following Reaſons: *Firſt*, The Structure of the Parts and its Diſtance from the Fang make it unlikely to be deſign'd for ſeparating the *poiſonous Fluid*, but rather a *Saliva* to moiſten the Aliment, in order to make it paſs down the *Oeſophagus* with Eaſe, the Stomach of thoſe Animals being but ſmall, and the Gullet conſiderably larger; not without ſome Analogy to the *Ingluvies* or Crop of Granivorous Fowls, where the Food ſtops for ſome time and is moiſtened, before it is capable of deſcending into the Stomach. *Secondly*, Theſe Parts are ſo contrived, that on opening the Mouth to receive the Prey (at which Time ſuch a Fluid is moſt wanted) the Muſcle above mentioned preſſing on the Gland promotes the Diſcharge of its Contents into the Mouth. The *Duct* of this Gland ſeems to open between the Upper Lip and the Jaw, but as the excretory Ducts of ſo ſmall a Gland are rarely to be ſeen with Certainty, I won't pretend exactly to determine its Aperture. Under this Gland lies another *Muſcle* ſmaller than the former, which ariſes and is inſerted near it (See *Fig.* 1. C.) theſe two Muſcles draw the Bone (*Fig.* 1. D.) in which

which the poiſonous Fang is fix'd a little outwards and upwards. Between the laſt deſcribed Muſcle and Gland paſſes a *Nerve* to the upper Part of the Bone which receives the Tooth (*Fig.* 1. E. and *Fig.* 2. B.) and it is probable that this *Nerve* has been taken for the *excretory Duct* of the Gland before mention'd. Opening the Mouth, two ſmall *Eminencies* appear in the Fore-part on the Inſide of the upper Jaw, being a *Membrane*, raiſed by the Fangs and drawn over them like the Mouth of a Purſe (*Fig.* 3. A. B. *Fig.* 2. C.) This Membrane is thick and ſtrong, and placed in a Microſcope, appears to have a Number of Glands, ſome of which are even viſible to the naked Eye. In a common Viper I obſerved one on each Side the Fang. Theſe Membranes prevent the involuntary Diſcharge of the Poiſon out of the Fangs (which in my Opinion are the *only Repoſitories* of that Fluid) into the Mouth, as alſo the killing with the Fangs little Animals on which they ſometimes feed. Putting back this Membrane, the *fatal Fangs* appear, which on firſt View ſeem'd to be only *one* on each Side, till ſearching further there appeared *four* more; the firſt and largeſt is fix'd in a *Bone*, which Bone is articulated to the fore Part of the upper Jaw (*Fig.* 1. F.) The four others are faſtened in and covered with ſtrong tendinous Membranes, and lie as it were one over another (*Fig.* 2. B. *Fig.* 3. C. and E.) Theſe Teeth are crooked and bent in this Form ⟩, eſpecially the firſt, and have each *two Perforations*, the one on the upper Part, the other at the lower Part of its convex Side; which laſt comes quite to the Point, and reſembles the ſloping Cut of a Pen. The upper Perforation (*Fig.* 4. A.) I imagine receives the Poiſon, the other tranſmits it into the Wound (*Fig.* 4. B.) All theſe Fangs are *tubular*, the

largeſt of which contained a ſmall Quantity of a *tranſparent Fluid* of a light yellowiſh Colour, which on putting the Snake into Spirit of Wine changed to a beautiful Red (the Fangs of the common Vipers I have examined had the lower Perforation nearer the Middle) Freeing the Mouth of the Membrane, a *Muſcle* appears about the Size of the firſt deſcribed above, which ariſes from the Middle of the *Maxillarum Dilatores* (*Fig.* 3. D.D.) and is inſerted on the under Side of the largeſt Tooth, for the Force required to pull down the Fang being leſs than to raiſe it, fewer Muſcles are required. This Animal was in my Cuſtody about a Month, during which Time he bit three Dogs, and a Cat; the two firſt were bit at the *College of Phyſicians*, and of theſe the firſt died about two Minutes after the Bite, and the Moment he was bit he grew convulſed, and loſt the Uſe of his Limbs. The Wounds were exceedingly ſmall, and between the pectoral Muſcles. Upon opening the Dog, the Skin and *Membrana Adipoſa* for the Breadth of a Crown were livid about the Wound, as if from a violent Blow. The ſecond Dog bit at the *College* had the ſame Symptoms with the firſt, but liv'd near a Quarter of an Hour, and had bloody Stools. Three Days after, I carried the Snake to bite another Dog and Cat. The Dog was larger than either of the two former, and having been bit at the Extremity of the Noſe he was immediately affected, howled, ſhook, fell down and foamed at the Mouth; and in about ten Minutes diſcharged his Excrements involuntarily, tinged with Blood: He died in about two Hours. The next Day I opened the Body and obſerv'd the *Abdominal* Contents very much inflamed, eſpecially the Stomach and Inteſtines, which appeared nearly equal to the

the fineſt Injection; opening the Stomach and Inteſtines they contained a mucous Matter, the greateſt Part of which was Blood, and the fine *villous* Coat which is ſo viſible in theſe Animals was entirely deſtroyed. About an Hour before he was bit he had a plentiful Meal of coarſe Beef, of which there was not the leaſt Appearance. Opening the *Thorax*, the *Pleura* and other Membranes looked as if injected; the Heart was turgid with Blood, as were alſo its Veſſels. The Veſſels of the Membranes of the Brain made a moſt beautiful Figure from the Quantity of Blood contained in them, as did likewiſe the Blood-Veſſels of the Nerves; there was a ſmall Quantity of Water between the two Hemiſpheres. The Blood contained in the Heart and its Veſſels was an even Maſs about the Conſiſtence of Cream. The Cat had upon opening nearly the ſame Appearances, and lived about five Hours.

II. *A Letter from the Rev. Dr.* Samuel Clarke *to Mr.* Benjamin Hoadly, *F. R. S. occaſion'd by the preſent Controverſy among Mathematicians, concerning the Proportion of* Velocity *and* Force *in* Bodies in Motion.

SIR,

IT has often been obſerved *in general*, that *Learning* does not give Men *Underſtanding*; and that the abſurdeſt Things in the World have been aſſerted and maintained, by Perſons whoſe Education and Studies

dies ſhould ſeem to have furniſh'd them with the greateſt Extent of Science.

That Knowledge in many *Languages* and *Terms* of *Art*, and in the Hiſtory of *Opinions* and *Romantick Hypotheſes* of Philoſophers, ſhould ſometimes be of no Effect in correcting Mens *Judgment*, is not ſo much to be wonder'd at. But that in *Mathematicks* themſelves, which are a *real Science*, and founded in the *neceſſary Nature of Things*; men of very great Abilities in *abſtract* Computations, when they come to *apply* thoſe Computations to the *Nature of Things*, ſhould perſiſt in maintaining the moſt *palpable Abſurdities*, and in refuſing to ſee ſome of the moſt *evident and obvious Truths*; is very ſtrange.

An extraordinary Inſtance of this, we have had of late Years in very eminent Mathematicians, Mr. *Leibnitz*, Mr. *Herman*, Mr. *'sGraveſande*, and Mr. *Bernoulli*; who (in order to raiſe a *Duſt* of Oppoſition againſt Sir *Iſaac Newton*'s Philoſophy, the Glory of which is the *Application* of *abſtract Mathematicks* to the *real Phœnomena of Nature*,) have for ſome Years inſiſted with great Eagerneſs, upon a Principle which ſubverts all Science, and which may eaſily be made appear (even to an ordinary Capacity) to be contrary to the *neceſſary and eſſential Nature of Things*.

What they contend for, is, That the *Force* of any *Body in Motion*, is proportional, not to its *Velocity*, but to the *Square of its Velocity*.

The *Abſurdity* of which Notion, I ſhall firſt make appear, and then ſhew *what* it is that has led theſe Gentlemen into Errour.

In the Nature of Things, 'tis evident, every *Effect* muſt neceſſarily be *proportionate* to the Cauſe of that Effect;

Effect; that is, to the *Action* of the Cauſe, or the *Power exerted* at *the Time* when the Effect is produced. To ſuppoſe any *Effect* proportional to the *Square* or *Cube* of its *Cauſe*, is to ſuppoſe that an *Effect* ariſes *partly* from its *Cauſe*, and partly from * *Nothing*.

In a *Body in Motion*, there may be conſidered diſtinctly, the *Quantity of the Matter*, and the *Velocity of the Motion*. The *Force* ariſing from the *Quantity of the Matter* as its *Cauſe*, muſt neceſſarily be *proportional* to the *Quantity of the Matter*: And the *Force* ariſing from the *Velocity of the Motion* as its *Cauſe*, muſt neceſſarily be *proportional* to the *Velocity of the Motion*. The *whole Force* therefore ariſing from theſe *two Cauſes*, muſt neceſſarily be *proportional* to theſe *two Cauſes taken together*. And therefore in Bodies of *equal Bigneſs and Denſity*, or in *one and the ſame Body*, the *Quantity of Matter* continuing always the *ſame*, the *Force* muſt neceſſarily be always *proportional* to the *Velocity of the Motion*. If the *Force* were as the *Square* of the Velocity, all that *Part* of the *Force*, which was *above* the *Proportion* of the *Velocity*, would ariſe either out of *Nothing*, or (according to Mr. *Leibnitz*'s Philoſophy) out of ſome

* Which is juſt *like* the *Suppoſition* made by thoſe Mathematicians, who have taken it for granted, that $\frac{1}{0}$ is equal to *Infinite*; that is, that as 0 to 1, ſo 1 is to *Infinite*; that is, that *Infinite* multiplied by 0, is equal to 1, or an infinite Number of *Nothings* equal to *Something*; which is palpably *falſe*. The *true Proportion* is, not as 0 to 1, ſo 1 to *Infinite*; but as *an Infiniteſimal* is to 1, ſo is 1 to *Infinite*. And as the *Infiniteſimal of an Infiniteſimal* (that is, a ſecond Fluxion, or the *ſecond Power of Infiniteſimal*) is to 1, ſo is 1 to *Infinity of Infinites*, or the *ſecond Power of Infinite*; that is, (for Inſtance) 'tis as a *finite* [*phyſical*] *Line* to an *Infinite Surface*, or as a *finite* [*phyſical*] *Surface* to an *infinite Solid*. And as 0 (which is *beyond all Proportion lower* than the *Infinit'th Power of an Infiniteſimal*) is to 1, ſo is 1 to that which is *beyond all Proportion higher* than the *Infinit'th Power of Infinite*. Which clearly removes the Foundation of all the *ridiculous* Conſequences, which have been drawn from the Suppoſition of the fore-mention'd *falſe* Proportion.

living

living Soul eſſentially belonging to *every Particle of Matter.*

Whenever *any Effect* whatſoever, is in a *duplicate Proportion*, or as the *Square* of any *Cauſe*; 'tis always either becauſe there are *two Cauſes* acting at the *ſame Time*, or that *one and the ſame Cauſe* continues to act for a *double Quantity of Time.*

The *Reſiſtence* made to a Body moving in any *fluid Medium*, is in a *duplicate* Proportion to the *Velocity* of its Motion; becauſe, in Proportion to its *Velocity*, it is reſiſted by a *greater Number of Particles* in the ſame Time; and again, in Proportion to its *Velocity*, 'tis reſiſted by the *ſame Particles ſingly* with a *greater Force*, as being to be moved out of their Places with *greater Velocity.*

Light decreaſes in a *duplicate* Proportion of its Diſtance from the Sun; becauſe the Rays *divaricate* according to *two* Dimenſions; according to the Dimenſion *upwards* or *downwards*, and according to the Dimenſion *ſide-ways.* But according to the *third* Dimenſion *forwards* from the Sun, a Ray of Light undergoes no Alteration; becauſe the Particles, of which it conſiſts, being emitted all of them with an equal Velocity, continue every where at an equal Diſtance from each other.

One and the *ſame* Cauſe, acting in a *double* Quantity of Time, produces the *ſame* Effect, as *two* equal Cauſes acting in a *ſingle* Quantity of Time. One and the ſame *Force*, in *two* Parts of Time, will cauſe a Body in Motion to deſcribe the *ſame Space*, as *double the Force* would do in *one* Part of Time. The *Space deſcribed* therefore by a *Body in Motion*, is not as the *Force*; but as the *Force* and the *Time* taken *together.*

A Body, with any the *leaſt aſſignable Force*, will move through *infinite Space*, if it meets with *no* Reſiſtence, in an *infinite Time*. And in Spaces where there is an *uniform* Reſiſtence to Motion, the *Space deſcribed* before the Motion ceaſes, muſt needs be as the *Force* and as the *Time* together: Becauſe a *double Force* will carry a Body *twice as far* in the *ſame Time*, and will alſo cauſe the Motion to be *twice as long Time* in *deſtroying* by an uniform Reſiſtence. The *Space deſcribed* therefore before the Motion ceaſes, is in this Caſe demonſtrably as the *Square of the Force*. A Body *thrown upwards* with *double Force*, will be carried *four Times* as high, before its Motion be ſtopp'd by the uniform Reſiſtence of Gravity; becauſe the *double Force* will carry it *twice as high* in the *ſame Time*, and moreover require *twice the Time* for the uniform Reſiſtence to deſtroy the Motion. The Caſe is the ſame in *accelerated* Motion; in Bodies *accelerated* by a Succeſſion of elaſtick Impreſſions, or *falling* with a Motion accelerated by the uniform Power of Gravity, or by any other uniform Power whatſoever. The *Space deſcribed* muſt needs be as the *Force*, and as the *Time wherein* the Force operates.

What I have thus demonſtrated concerning *any Force*, conſidered as the *Cauſe* producing an Effect; and concerning the *Time*, during which the Force operates; is on all Hands acknowledged to be true concerning *Velocity*. And therefore *Velocity* and *Force*, in this Caſe, are one and the ſame Thing. So that to affirm *Force* to be as the *Square* of the *Velocity*, is to affirm that the Force is *equal* to the *Square* of *it ſelf*.

Now from hence appears very clearly the *Ground* of the *Errour* theſe Gentlemen have fallen into, and

of their *Miſapplication* of the Experiments they build upon.

The *Effect* of a *Force* impreſs'd on a moveable Body, is the *Motion* of that Body from one Place to another. Now foraſmuch as the *Effect* cannot but be proportional to its *Cauſe*, hence Mr. *Leibnitz* (whom the other Gentlemen have follow'd) contends that the *Space deſcrib'd* by a Body in falling, is proportional to the *Force* by which it is impell'd during its Fall; and that the *Force* acquir'd by a Body in falling, is proportional to *the Space it has deſcribed* in its Fall. Which *Space* being agreed to be as the *Square of the Velocity* (as being proportional to the *Velocity* and to the *Time* taken *together*) hence they infer that the *Force* likewiſe is as the *Square of the Velocity*.

But from what has been ſaid, 'tis plain, that the *Space deſcribed* in theſe and all other the like Caſes, is not as the *Force* only, but as the *Force* and as the *Time wherein* the Force acts; that is to ſay, as the *Square of the Force*. For the *Cauſe* of the Quantity of the *Space deſcribed*, is not barely *the Quantity of the Force*, but alſo the *Continuance of the Time* wherein the Force acts. The *Force* therefore and the *Time* taken *together*, being neceſſarily as the *Space deſcribed*; as the *Velocity* and the *Time* taken *together*, are on all Hands acknowledg'd to be; it follows that the *Velocity* and the *Force* are equal, and not the *Force* as the *Square of the Velocity*.

When two *unequal* Bodies faſtened at the Ends of the *Arms* of a Balance of *unequal Length*, counterpoiſe each other, and vibrate in *equal Times*; as they muſt neceſſarily do, being *faſtened* to the Arms of the *ſame* Balance: which is an Obſervation Mr. *Leibnitz* lays great Streſs upon: In *That* Caſe indeed the *Forces*

will be as the *Spaces deſcribed.* But not therefore as the *Squares of the Velocities.* For in *That* Caſe, the *Velocities themſelves* are as the *Spaces deſcribed,* becauſe the *Times* are equal.

When a Body projected with a *double Velocity*, enters *deeper* into *Snow* or *ſoft Clay*, or into a Heap of *ſpringy* or *elaſtick* Parts, than in Proportion to its *Velocity*; 'tis not becauſe the *Force* is more than proportional to the *Velocity*; but becauſe the *Depth* it penetrates into a ſoft Medium, ariſes partly from the *Degree* of the Force or Velocity, and partly from the *Time wherein* the Force operates before it be ſpent.

In the Colliſion of *hard* Bodies, 'tis (I think) agreed on all Hands, that 'tis *demonſtrated* by *Reaſon*, and confirmed by *Experience*; that when a perfectly hard Ball, moved with *whatever* Degree of *Velocity*, ſtrikes full upon another hard Ball, equal in Bigneſs and Weight, and without any Motion in it; if the Balls be *unelaſtick*, they will both go on together the ſame Way, dividing the Motion equally between them, with *half the Velocity* the firſt Ball had originally: But if they be perfectly *elaſtick*, the moving Ball will communicate its whole *Motion and Velocity* to the quieſcent Ball, and it ſelf *lie ſtill* in the others Place. Were it true now, that the *Force* of the *moving Ball* was as the *Square of its Velocity*; theſe Experiments would then ſhew (which is infinitely abſurd) that the *Force* or *vis inertiæ* in the *quieſcent* Ball, the *dead* Force, was always proportional to the *Square of the Velocity* (which theſe Gentlemen affect fantaſtically to call the *living* Force) of the *moving* Ball, whatever its Velocity were. Or the *Force* in *Both* might juſt as reaſonably be ſuppoſed to be as the *Cube*, or the *quadrato-quadrate*, or any *other Power* of the Velocity of the *moving* Ball. Which is turning the *Nature*

of Things into Ridicule. Mr. *Leibnitz*, in ſome Letters which he wrote into *England*, intimated that he had a Proſpect of a *perpetual Motion*, founded on the Notion of a *Vital Principle*, or *active Power* in *Matter*. But from the Experiments now mentioned, 'tis evident that if the *Force* of Bodies in Motion could be exalted even to the *infinit'th Power* of their *Velocity*; yet ſince, to anſwer the Phœnomena of Nature with Regard to *Action* and *Re-action*, the ſame *Force* muſt neceſſarily be allowed to all *quieſcent* Bodies likewiſe; it could be of *No Effect*.

III. *Aſtronomical Obſervations made at* Vera Cruz, *by Mr.* Joſeph Harris. *Reviſed and communicated by* Edm. Halley, L. L. D. Aſtron. Reg. & R. S. S.

THE Latitude of this Place I found (by ſeveral diſtant Obſervations made by a Quadrant of four Feet Radius) to be 19° 12′ N.

On *March* 11, 1727. *O. S.* there happen'd here a conſiderable Eclipſe of the Sun, the greateſt Obſcuration being about 10½ Digits; and having that Morning carefully adjuſted the Pendulum Clock, and fixed a Teleſcope to the Index of the foreſaid Quadrant, I obſerved it to begin in or about the *S. E.* by *S.* Part of the Sun's Disk at 49½ Minutes after Noon apparent Time; the Altitude of the Sun's Center then was 67° 53′.

We could not determine exactly the Middle of this Eclipſe, but as near as we could judge, it happen'd about $2^h\ 30^m$ P. M.

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The Eclipſe ended in or about the *N. N. E.* Part of the Solar Disk, at 3^h 59′ ½ P. M. at which Time the Sun's Altitude was 28° 34′.

By comparing theſe Obſervations with a Calculation which I made from Mr. *Flamſtead*'s Tables, I judg'd *Vera Cruz* to lie 97° 30′ to the Weſtward of the Meridian of the ſaid Tables.

We had here alſo a *Lunar* Eclipſe *September* 29. 1726, *O. S.* but it happen'd to be a little cloudy about the Time of its Beginning and Ending, ſo that we could not get a juſt Obſervation.

In the Years 1726, and 1727, I obſerved here the *Magnetick Variation* ſeveral Times, and found it to be about 2¼ Degrees Eaſterly.

We alſo obſerv'd the Variation ſeveral Times on our Voyage from *England* towards *Vera Cruz* (having on Board a good Azimuth Compaſs) but I always found that the beſt Obſervations we could make, when compared together, differ'd ſo much, that we could not depend upon them, to much leſs than three or four Degrees, or ſometimes half a Point of the Compaſs.

IV. *A new Method for compoſing a natural Hiſtory of* Meteors *communicated in a Letter to Dr.* Jurin, R. S. & Coll. Med. Lond. Soc. *By Mr.* Iſaac Greenwood, *Profeſſor of Mathematicks at* Cambridge, New-England.

Honoured Sir,

THIS Method in general is, that in Addition to ſuch Obſervations as ſhould be made on Land, there might be ſome Account taken of thoſe alſo that were made at Sea; which already are by far more numerous than what were ever made aſhore, or indeed what can be expected thence for ſome Ages ſtill to come. This Method occured to me, as I was looking over various *Journals of Voyages* in my Paſſage from *England*, in which I was not a little ſurpriz'd to find the following Particulars conſtantly obſerv'd.

Firſt, There was a general Account of the *Weather* for every Day, during the Paſſage of the Ship on the Voyage, which tho' not quite ſo exact as the Obſervations of the ſame Kind that have been made on Land, particularly what were publiſh'd by the Rev. Mr. *Derham*, yet for all that I know, are ſufficient for the Deſign. However, if there is any Defect in this Article, it is abundantly made up in another Column; which is a far more exact Regiſter of the *Direction of the Winds* than was ever kept *aſhore*, being an Account thereof to every two Hours in the Day. This Article may perhaps

haps be of very great Importance; ſince, as you obſerve, *Compertum habemus, ut quod maximè, ſubitas tempeſtatum commutationes* Ventis *præcipuè acceptas eſſe referendas*. As for the *Degree* or *Strength* of the Wind there are alſo ſufficient *Data* in all Sea-Journals to determine it, as I ſhall particularly ſhew in the Sequel of this Letter. Laſtly, there is a daily Account inſerted of the *Latitude* and *Longitude* of the Ship, that there will be no Difficulty in computing what Part of the Globe each Obſervation belongs to.

And now ſince there is in the World a great Variety of theſe *Marine Obſervations* already made, (for in all Voyages whatſoever that have been perform'd for many Years paſt, it has been cuſtomary to keep an exact Journal of the aforeſaid Articles,) I thought it might be no difficult Matter to collect therefrom the *Hiſtory* of the *Winds*, and *Weather* in moſt Parts of the Ocean.

In order to this, I imagin'd that if the *Royal Societies* of *London* and *Paris* ſhould encourage ſuch a Deſign, they might eaſily procure *Extracts* from moſt of the Journals kept in their reſpective *Nations*: For certainly ſuch Gentlemen as would be at the Pains to keep a conſtant *Diary* of the Weather, would not fail alſo to communicate ſuch *Marine Obſervations*, as they ſhould be able to obtain.

The *Seamen* likewiſe themſelves, (among whom there are a conſiderable Number of ſuch as have a Taſte for *phyſical* Knowledge) as they are under a Kind of Neceſſity to obſerve exactly the *Winds*, *&c.* would not be backward in tranſmitting their Obſervations; eſpecially when they were informed of what Importance and Advantage it might be to themſelves, and the Cauſe of *Navigation*.

I pro-

I proceeded further to think, that if the aforeſaid *Societies* ſhould judge it improper to be at ſo great an Expence as would be requiſite in printing ſo many Extracts from ſuch Journals as ſhould be ſent to them, that they might notwithſtanding keep in *Manuſcript* a Book of Tables of ſuch *Marine Obſervations*, as they ſhould think fit to collect therefrom; and that the *Secretaries* of the Society (who for the moſt Part are ſuch Gentlemen as have in a particular Manner diſcover'd a generous Principle of promoting Natural Knowledge) ſhould take Care, that all ſuch Obſervations were tranſcribed in their proper Places.

The Form of theſe *Tables* I thought might be in the following Manner.

January the Firſt, 1726.

Longitude.		20.			21.			22.		
Latitude	H.	W.	D.	Weather.	W.	D.	Weather.	W.	D.	Weather.
	12.	N.	2.	Fair.						
	6.	——		——						
	12.	N b E	3.	Fair.						
30.	6.	——		——						
	12.							S b E	2.	Cloudy.
	6.							——	2.	Rain.
	12.							S	3.	Stm. of Rain.
31.	6.							S b E	2.	Rain.

In which the *Title* ſhews the *Year*, *Month*, and *Day*; the *horizontal Space* juſt below it, the *Longitudes*; the *vertical Space* without the double Lines, the *Latitudes*; *that within the double Lines*, the *Hour* of the Day; and the *horizontal ſpaces* under the Longitudes,

tudes, the *Wind*, its *Degree*, or *Strength*, and the *Weather*, which are accordingly mark'd with W. D. Weather.

In this Specimen I have noted every Degree of Latitude and Longitude, that the Work might be the more perfect. I have only taken Notice of Four Hours in the Day, *viz.* 12 at Noon, 6 in the Afternoon, 12 at Night, and 6 in the Morning. However, if there be requir'd a greater Exactneſs in this Article, it will be eaſy enough to frame Tables accordingly. I began the Hours with 12 at Noon, becauſe all Journals are kept from that Period, the Marine Day being always counted from Noon to Noon. There may be other Columns inſerted, as I ſhall mention in the Cloſe of this Letter, tho' what I have already taken Notice of is ſufficient to our preſent Deſign.

Of theſe Sort of Tables there muſt be at leaſt Four Volumes; One for the *Atlantick Ocean*, which will be by far the largeſt of them all, and perhaps the moſt profitable, as moſt of the Trade of the World lies thereon; the ſecond for the *Mediterranean*; and the others for the *German Sea* and *Baltick*. In Proceſs of Time alſo, it may perhaps be thought worth the while to preſerve ſuch Obſervations as are made on the *Indian Sea*, and the *Pacific Ocean*.

I muſt acknowledge my ſelf incapable of making a juſt Compariſon between the Advantages and Inconveniencies attending ſuch a *tabular Regiſter* of the Winds and Weather, as I have propoſed; however, I will take the Liberty to mention one or two Things, by which you will ſee how the Matter appears unto me.

It muſt be confeſs'd, that the Work will be very much protracted, and require ſome conſiderable Appli-

cation and Care in extracting ſuch Obſervations, as ſhall be of Uſe, from Journals. There will alſo be ſome Difficulty in procuring any conſiderable Number of ſuch Journals; and laſtly, there is but a very ſmall Number of Obſervations made, in Compariſon to the Spaces that muſt be allow'd in the Tables for them, by which Means there muſt neceſſarily be a great Waſte.

In Anſwer to theſe Objections, it may be ſaid in general, that there will be much leſs Application and Care requir'd than in keeping a *Diary* of the Weather, &c. on the *Land*. By this Means alſo, there may be more Obſervations collected in a few Years, than can be expected from the other Method in ſome Ages; and one Man may be able in a few Months, hereby to compile a larger Hiſtory of the Weather, than what has hitherto been done by the united Obſervation of all ſuch, as have undertaken this Province.

Tho' there might be ſome Difficulty as to *particular Perſons* in procuring a great Number of Journals, it cannot be ſuppos'd, that ſo illuſtrious a Body of Men as the *Royal Societies* at *London* and *Paris*, ſhould meet with the ſame. It is obſervable alſo, that in the *Royal Navy* of *Great-Britain*, the Maſters of the *Mathematicks* are oblig'd to keep ſuch a Journal by an Act in the late Reign, on Board every Ship, which without Doubt might be eaſily obtained on this Occaſion: Nor can we imagine any in the trading Intereſt would refuſe a Thing, that tended ſo much to their own Advantage and Benefit.

It is true, there can be no Remedy for the many empty Spaces in the Tables, (if that Method be follow'd which I have propos'd;) however this will be look'd upon as a trifling Objection, by ſuch as conſult the Improvement

provement of *Natural Knowledge*, rather than the Waſte of Paper.

I ſhall conclude theſe general Remarks, by obſerving, that as the Hiſtory of the Winds and Weather is capable of a more ſpeedy and expeditious Improvement from *Marine Obſervations* than from Diaries from the *Land*, ſo alſo it is capable hereby of a more large and extenſive Improvement. Without Doubt it will require many Years before *Obſervatories* of the Weather, *&c.* will be erected at all the *Univerſities* and *Capital Towns* of the *Provinces*, *Shires*, &c. in *Europe*, (if ever ſuch ſhould be,) not to mention *Africa*, *Aſia*, and *America*, from which little can be expected in this Affair; and yet upon that Suppoſition, how few would the Diaries be, in Compariſon of the great Number of Journals that are annually kept at *Sea?* beſides many Thouſands that might perhaps be obtain'd, relating to the Courſe of the Winds and Weather, ſucceſſively for many Years laſt paſt. It is beyond my Abilities, indeed, to calculate with any Exactneſs, how many Veſſels there may be upon the Seas which I have named, in the Space of one Year, and conſequently how many diſtinct Journals there are annually kept; however, if I may judge from the Trade of the little Town, where this Letter is dated, there muſt be many Thouſands: For there are ſeldom leſs than eight or nine Hundred Voyages made to and from this Port in a Year. I ſhall only add in this Place, that the Method here propos'd ſeems to have the Advantage of the common Method heretofore uſed in compoſing the *Natural Hiſtory of Meteors*; inaſmuch as *that* requires a particular Application and Attention without any other Views and Advantages; whereas in *our Caſe* there is a Kind of Neceſſity

of making ſuch Obſervations, in order to conduct a Ship ſafely thro' the Ocean, whether the philoſophical Part of Mankind ſhall think fit to improve them in their Intereſt, or no: However, I would not be underſtood, by any Thing that has been ſaid upon this Head, to derogate from the Deſign of obſerving *on Land*, for that likewiſe has many Advantages, that we can by no Means pretend to in the *New Method*.

I ſhould protract my Letter to an unreaſonable Length, did I enter profeſſedly upon the particular Advantages and Benefits, that may accrue from ſuch a *Tabular Regiſter* of *Marine Obſervations*, as I have propoſed. I ſhall mention therefore only two Things.

Firſt, We may be able from this Method to define with a great deal of Exactneſs, the Bounds and Limits of all conſiderable Winds; for as there are at all Times in the Year ſome Hundreds of Veſſels at Sea, it is of the ſame Importance in our Caſe, as tho' there were ſo many diſtant Obſervatories there; and that the Knowledge of theſe more *extenſive* and *general Winds* would be of conſiderable Uſe, none will deny, that ſhall attentively conſider it; for hereby we may be able to judge, in what Place ſuch a Wind has its Origin, how long a Time it continues, with what Velocity it moves, where its greateſt Strength is, and how great a Part of the Earth it paſſes over. Perhaps alſo, in Proceſs of Time, by this Means, we may arrive to ſo much Skill, as to judge with ſome conſiderable Certainty, from the Riſe or Beginning of a Wind, what its Effect and Iſſue ſhall be; which will be of as great Importance in *Navigation*, as any Thing ſtill wanting. Again, from ſuch *Marine Obſervations* of the more extenſive and laſting Winds, it is not impoſſible, that we ſhould be able to

make

make a probable Judgment of the Effect and Influence of the Wind upon the Weather; which, for what Cauſe I know not, I have frequently obſerved at Sea, to change and alter, according as that doth.

Secondly, From collecting all ſuch *Meteorological Obſervations* as are made at *Sea*, we may reaſonably expect to come to the Knowledge of ſuch Winds, as prevail moſt in particular *Latitudes*. Tho' the Wind is a very uncertain Meteor, there is no Doubt, but that in ſome Places, it has a very different Courſe from what it has in others. If I miſtake not alſo, it has been frequently obſerved, in ſome particular Places, that the Courſe of the Wind in one Year has been much the ſame as in others; and tho' there has been no particular Order or Exactneſs yet diſcover'd, yet the *prevalent Winds*, or the greater Number of Winds have been, in both Caſes, according to the ſame Direction: In theſe Parts of the World it is remarkably ſo. We can't, indeed, expect to diſcover the *Reigning* or *Prevalent Winds* of ſuch *Latitudes*, as are very diſtant from the *Tropicks*, by as eaſy an Obſervation, as the *Trade-Winds* and *Monſoons*, which are in the *Torrid Zone*, were firſt found out. However as it has been after many Obſervations, that the Courſe of thoſe *Fix'd Winds* was determin'd, we may alſo hope, that Time and Induſtry may bring us to a much better Knowledge than what we have at preſent, of theſe which are more *Variable*. I need not ſay of how much Importance it would be to the Trading Part of the World, were we able to define the more frequent and reigning Winds of every Climate; for as the Probability of Voyages might then be calculated in the ſame Manner as that of other Chances, the Sailor might then better know

know how to order his Courſe ſo, as to arrive with the moſt probable Diſpatch to his Port.

It may not be impoſſible alſo, from a protracted Series of *Sea-Obſervations*, not only to know the general Courſe of the Winds in every Climate in the whole Year, but alſo to make a very probable Judgment of the reigning Winds of the ſeveral *Seaſons of the Year*, and perhaps of *every Month* too: Which if it could once be obtained, we ſhould have nothing more uncertain in *Navigation*, than that it was a *Doctrine of Chances*, which might be mathematically calculated.

I ſhall mention under this Head but one thing more, which we may with all the Probability imaginable expect to arrive to, *viz.* the particular Seaſons, Signs, and Places of the *Tornados* and *Hurricanes*. The Effect of theſe are in many Caſes ſo fatal, that they call for all our Skill and Obſervation: And could the Hiſtory hereof be ſo ſucceſſively known, as that we might be able to draw any certain Concluſions from it relating hereunto, it might perhaps be a ſufficient Recompence for all the Care, that is requir'd, in the whole Collection of *Marine Obſervations*.

I might add in common to the two foregoing Heads, that the *Marine Obſervations* have much the Advantage of ſuch as are made *on Land*, (which notwithſtanding are of very great Service,) inaſmuch as they are not obnoxious to any external Accidents, as theſe are; the Winds aſhore being frequently interrupted in their Courſe, and often diverted therefrom, by intervening Mountains, Vallies or Promontories.

Were I allowed to reckon among the Advantages of this *New Method* of obſerving on the Winds and Weather, thoſe incidental Obſervations, that might be found

in

in *Journals*, of general Benefit to Mankind, they are perhaps alone ſufficient to engage us in the Work. I ſhall only hint here, that if it ſhould be thought proper to practice our Deſign, it may perhaps be worth the while to inſert into the *Meteorological Tables*, ſuch Obſervations as relate to the *Variation of the Compaſs* and *Currents*; the true Knowledge of which would be of no inconſiderable Service to *Navigation*.

If likewiſe there was a Column left for ſuch remarkable Accidents as did occur, it might not be amiſs; particularly, any uncommon Diſcoveries of Lands, Rocks, or Soundings; exceſſive Thunder and Lightning, *&c.* Luminous Appearances in the Sky; what Remarks may be found relating to the Water-Spout, which tho' perhaps one of the moſt curious Phænomena of Nature, is as little known as any whatſoever; ſubmarine *Hiatus* or Wirlpools, if any ſuch there be; and laſtly, any extraordinary Rendezvous of Fiſh, *&c.* that are uſed in the Affairs of Life, not to mention ſuch Deſcriptions, as may relate to Matters of meer Speculation and Curioſity.

But theſe Sort of accidental Advantages, in ſuch a Collection of Journals of Voyages, as is neceſſary to our Deſign, are too numerous to be inſiſted on: I ſhall therefore only add one more, which is the great Improvement there would hereby be given to *Geography*, a Science of the greateſt Uſe and Importance in the Affairs of Life. Not only all *Hydrographical Charts* might be by this Means corrected, and brought to the Truth, which is of ſo much Concern, that the Lives of a great Part of ſuch as go to Sea depend upon it; but alſo, the Diſtances and Situation of all *Sea-Ports*, and many other Things, which are uncertain, or wanting in that Science, determin'd with the greateſt Exactneſs.

In a Word, *Geography* may, by ſuch an Expedient, arrive, in a very expeditious Manner, to as great a Degree of Perfection, as it is capable of.

I ſhall have finiſh'd my Deſign in this Letter, when I have taken Notice of the Method of determining the Degree and Strength of the Wind, from ſuch *Data* relating thereunto, as are to be found in Sea-Journals; which in general is, from obſerving how many *Knots* the Veſſel goes at the Time of Obſervation; which is always inſerted in the *Day-Book* or *Journal*; or, in other Words, what *Velocity* ſhe then has; for the Strength of the Wind may, with Exactneſs enough, in this Affair, be judg'd of from the Effect it produces, or the Motion it communicates to the Ship. It is true, there will be ſome conſiderable Difference in this Reſpect, ariſing from the Shape and Burthen of the Veſſel: However, as we do not expect a mathematical Exactneſs in this Article, after a little Uſe and Experience, together with comparing the greateſt Velocities of different Ships together, a Perſon may ſeldom fail of judging of the Strength of the Wind, at leaſt to a fourth Part; that is, if according to the Method you propoſe in your *Invitatio ad Obſervationes Meteorologicas*, the greateſt Winds be expreſs'd by 4, and the lighteſt by *Unity*.

In *Oblique Winds*, the Strength or Degree thereof will not be directly proportional to the Velocity of the Veſſel, but muſt be corrected a little; however, there will be no Difficulty in this Matter. For ſuch as are acquainted with the Method of reſolving *Oblique Powers* into *Direct ones*, may eaſily compoſe a Table of *Proportional Parts* ſuited thereunto. I did intend to have inſerted ſuch a Table; but I am afraid I have already

already treſpaſs'd in the Length of this Epiſtle, and ſhall therefore only add, at preſent, that I am,

With the utmoſt Sincerity and Reſpect,

Your moſt obedient, and

devoted humble Servant,

Iſaac Greenwood.

POSTSCRIPT.

SINCE my writing the foregoing, I have thought on a *Method* of keeping the *Meteorological Tables* therein propoſed, whereby the Work will be very much contracted. This is, inſtead of conſtructing the *Tables* according to the *Oceans* whereon the moſt conſiderable Trade of the World lies, to frame them only according to the *Route* of the moſt common *Voyages* on thoſe *Seas*. Thus, may one of the *Tables* be confin'd to that Part of the *Atlantick Ocean*, which ſuch Ships generally paſs over, that Trade between *Great-Britain* and the *Weſt Indies*; another *Table* for thoſe Parts of the *Ocean*, that lie in the Paſſage of ſuch Ships as are engaged in the *Mediterranean*, or *Turkey Trade*; to which may be added a *Table* for the *African*, and *India Commerce*. Another *Table* may be framed to that Part of the *Ocean*, that lies between the *Northern Provinces* in *America* and the *Weſt Indies*; as alſo one for the Ships that paſs between *New-England*, and

New-York and *Britain*, which on the Northern Part may be made ſo wide as to take in the *Newfoundland* Trade, *&c.* But I ſhall mention no more Particulars; for in the ſame Manner it will be eaſy to conſtruct *Tables* to all thoſe conſiderable Parts of the World, to which Voyages are perform'd.

V. *Some Obſervations towards compoſing a Natural Hiſtory of* Mines *and* Metals, *communicated in a Letter to Dr.* Rutty, S. R. Secr. *&* Coll. Med. Lond. Soc. *By Dr.* Frank Nicholls, Profeſſor *of* Anatomy *at* Oxford.

Dear Sir,

IN Obedience to your Commands, I here ſend you the Particulars of what I obſerv'd during a Year's Stay in the Weſtern Part of *Cornwall*, concerning *Mines*, &c.

Mines in general are Veins or Cavities within the Earth, whoſe Sides receding from, or approaching nearer to each other, make them of unequal Breadths in different Places; ſometimes forming large Spaces, which are call'd Holes. They are fill'd with Subſtances, which, whether metallick, or of any other Nature, are term'd the *Loads*. When the Subſtances forming theſe Loads are reducible to Metal, the Loads are by the Miners ſaid to be *alive*; otherwiſe they are term'd *dead Loads*.

In

Plate 2.

Fig. 12.

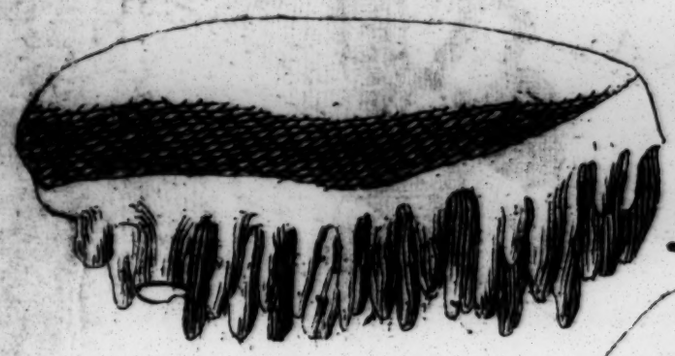


Fig. 9.

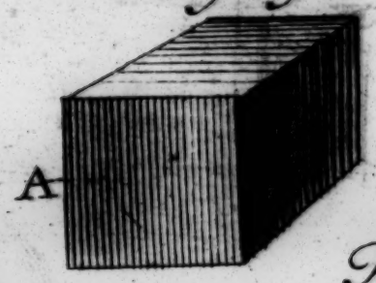


Fig. 10.

Fig. 11.

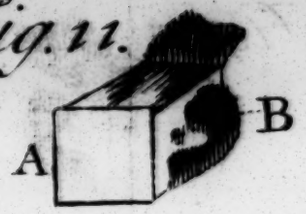


Fig. 13.

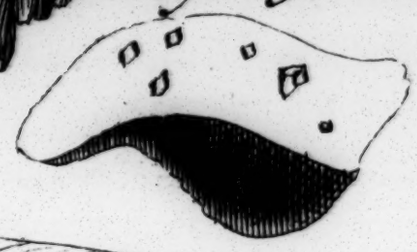


Fig. 7.

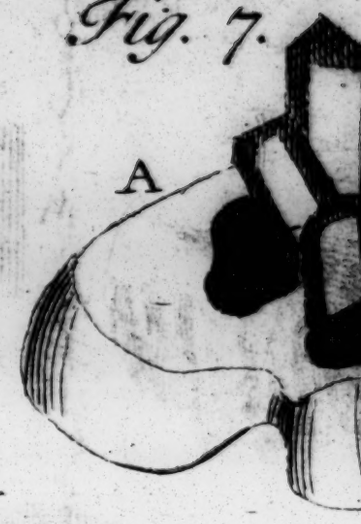
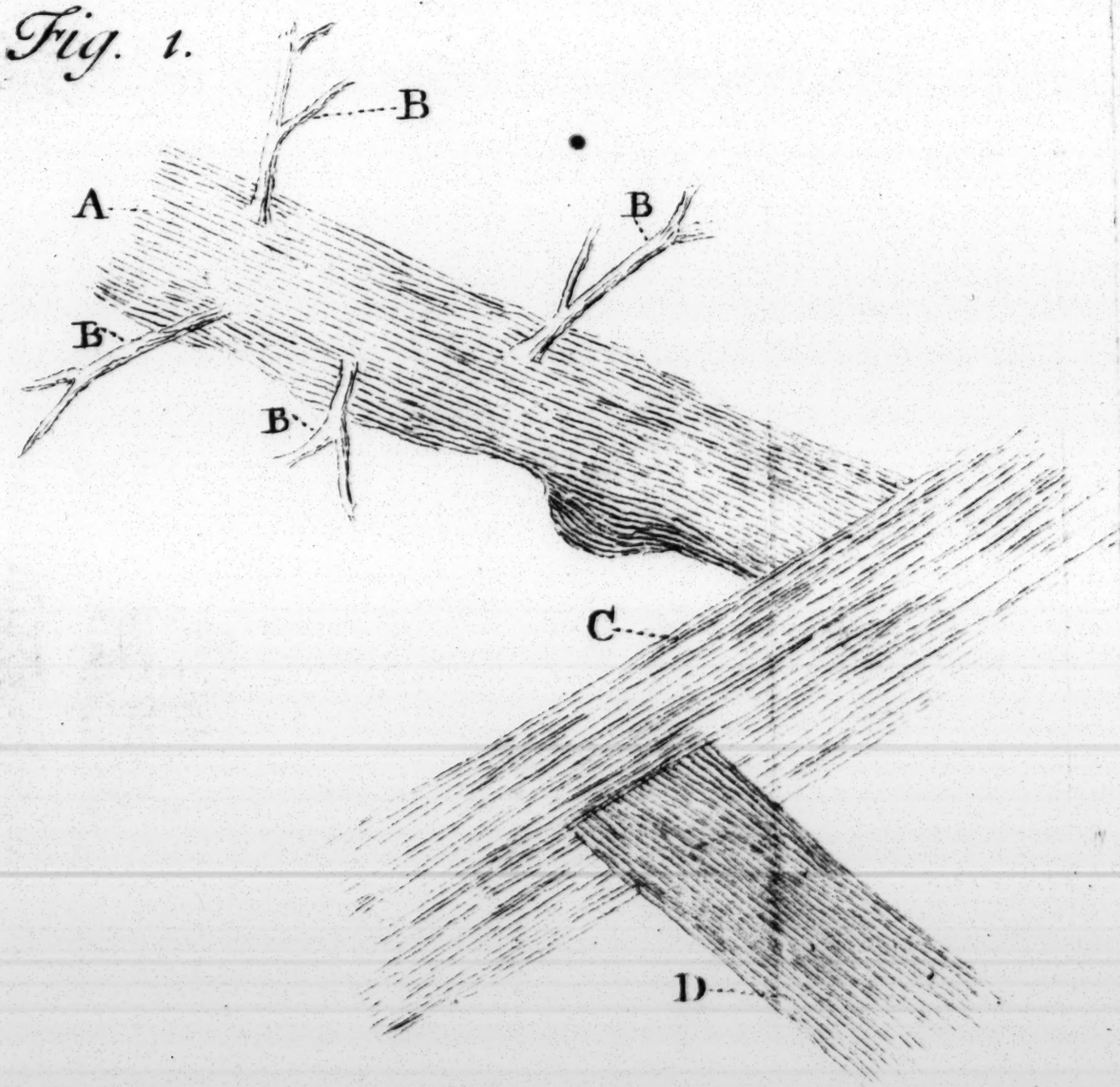


Fig. 1.



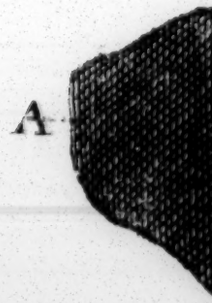


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Fig. 8.

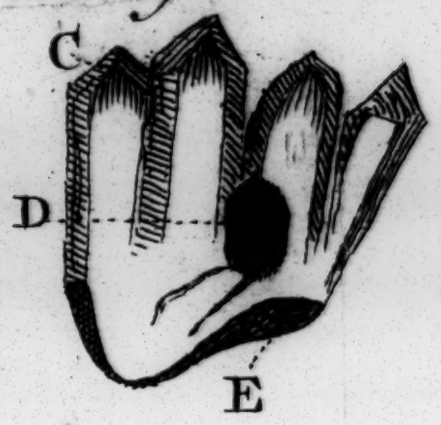
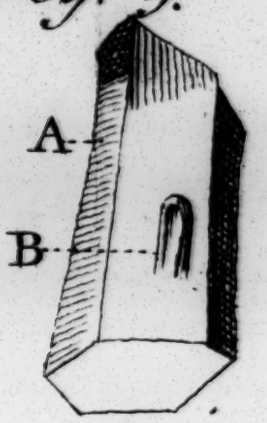


Fig. 5.



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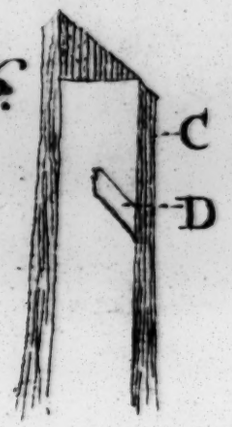


Fig. 4.

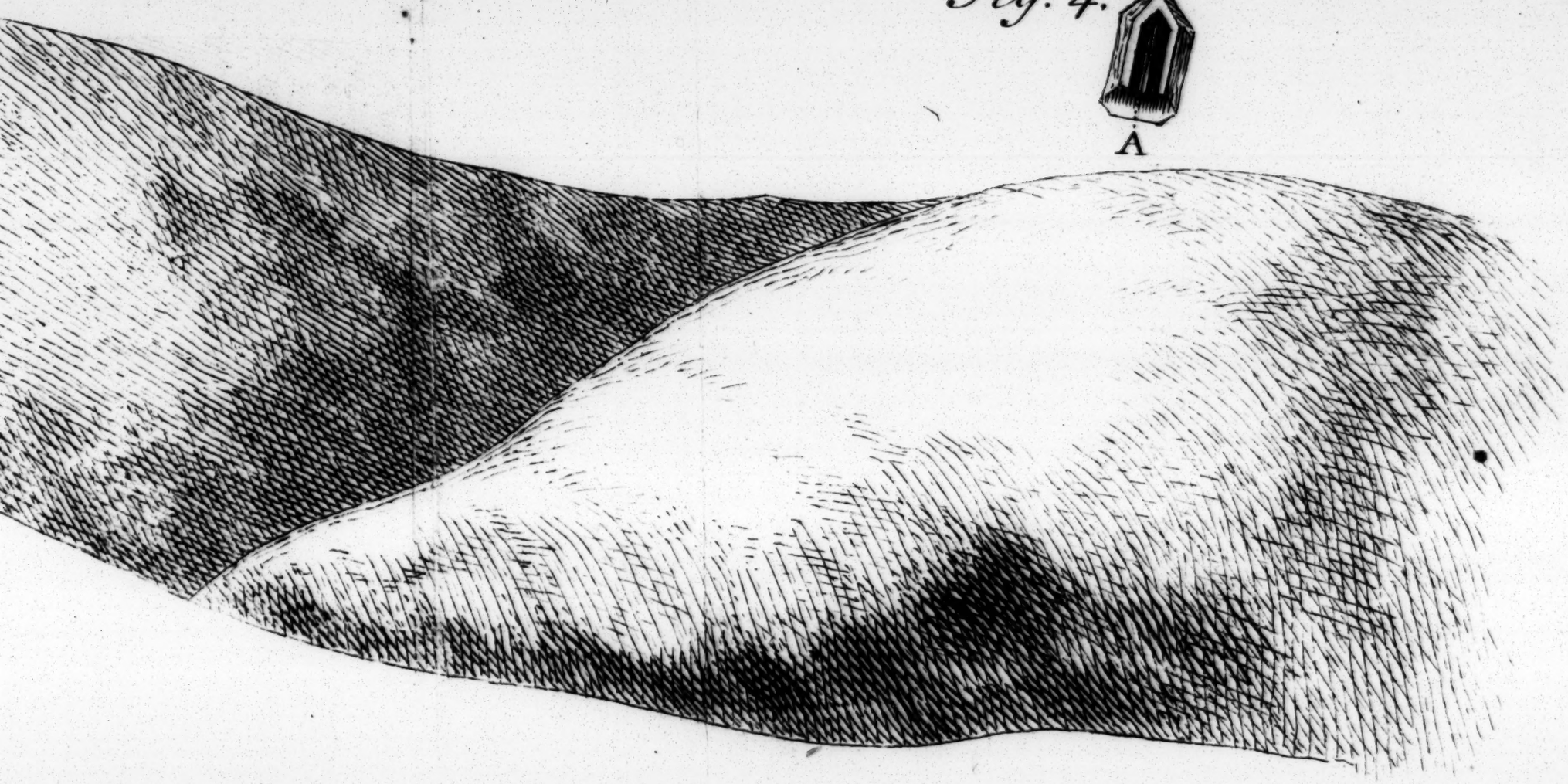
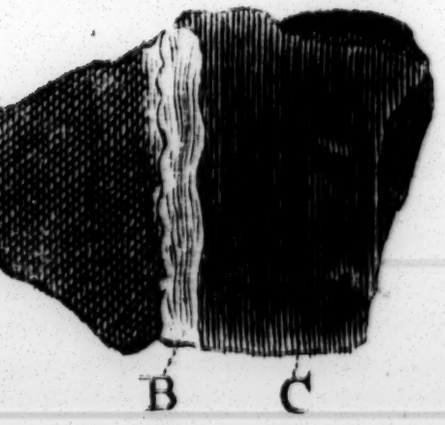


Fig. 3.

Fig. 2.



BRITISH MUSEUM

In *Cornwall* and *Devon* the Loads always hold their Courſe from Eaſtward to Weſtward; tho' in other Parts of *England* they frequently run from North to South. The Miners report, that the Sides of the Load never bear in a Perpendicular, but conſtantly underlay either to the North or South.

The Mines ſeem to be, or to have been, the Channels thro' which the Waters paſs within the Earth; and, like Rivers, have their ſmall Branches opening into them in all Directions; which are by the Miners term'd, the *Feeders* of the Load.

Moſt Mines have Streams of Water running thro' them, and when they are found dry, it ſeems to be owing to the Waters having changed their Courſe, as compell'd to it, either becauſe the Load had ſtopp'd up the antient Paſſages, or that ſome new and more eaſy ones are made.

The Load is frequently intercepted by the croſſing of a Vein of Earth, or Stone, or ſome different metallick Subſtance. In which Caſe it generally happens, that one Part of the Load is moved a conſiderable Diſtance to one Side. This tranſient Load is by the Miners term'd a *Flooking*; and the Part of the Load which is moved, is, in their Terms, ſaid to be *heaved*. This heaving the Load would be an inexpreſſible Loſs to the Miner, did not Experience teach him, that, as the Loads always run on the Sides of the Hills, ſo the Part heaved is always moved towards the Deſcent of the Hill. So that the Miner working towards the Aſcent of the Hill, and meeting a *Flooking*, conſiders himſelf as working in the Part heaved; wherefore cutting thro' the *Flooking*, he works upon its Back towards the Aſcent of the Hill, till he recovers the Load, and *vice verſâ*.

Thus in *Figure* the *firſt*, A D ſhews a Load running in the Side of a Hill, B the *Feeders*, C the *Flooking*, D is the Part *heaved*.

Sometimes, tho' not conſtantly, the Mine is lined with an intermediate Subſtance between the Load and it ſelf. This is (properly ſpeaking) the Wall of the Load: Though, in the common Acceptation of that Term, it ſignifies either ſuch intermediate Subſtance, or the Side of the Mine, where the Load immediately unites it ſelf to it. Thus in *Figure* the *ſecond*, A is the Side of the Mine, B the intermediate Wall of white Mundick, C the Load of Copper. And in *Figure* the *third*, A B the two Walls of *Spar-Stone*, C a ſmall Vein of *Tin Oar*.

The Springs in theſe Parts are always hard, as abounding very much, either in ſtony, or *ſulphureo-ſaline* Particles.

From this Water thus ſaturated with ſtony Particles, we frequently find the Paſſages of the Water under Ground, either partly, or totally ſtopp'd up; the ſtony Matter gradually concreting round the Sides of the Mine, and forming thereby a confuſed Load of Spar-Stone.

At other Times this ſtony Matter concretes more diſtinctly: In which Caſe the ſtony Matter ſeems to be govern'd in its Concretion by a *Plaſtick* Power.

N. B. When I ſpeak of a *Plaſtick* Power, I would be underſtood as meaning only a *Modus* of *Attraction*, by which the attracted Particles are rang'd in this or that determin'd Form. This Power then ſo exerts its Action, as to range the concreting Matter into the Form of an *hexagonal Priſm*, whoſe Head goes off in an

hexagonal Pyramid. Where this plaſtick Power happens to be ſingle and uncontroul'd, it preſerves the Form of the Criſtal to very conſiderable Magnitudes.

In theſe ſingle Criſtals we may obſerve, that they are of different Tranſparencies and Colours, as the ſtony Matter is more or leſs diſengaged from other Subſtances, or as thoſe other Subſtances are capable of imparting different Tinctures to them. And that they ſeem form'd *laminatim*; tho' the *Laminæ* are only diſtinguiſhable, when the Matters from whence the Criſtal is ſucceſſively form'd, happens to differ in Purity. Thus in *Figure* the *fourth*, the Criſtal was at firſt form'd from Matter intangled with a foul yellow Subſtance; after which, a pure Matter advening, the Criſtal was in its future Lamination form'd more pure and tranſparent.

But where the plaſtick Particles are more numerous, there ſeems Reaſon to believe, that theſe very *Plaſtick Particles*, before they are fix'd, are ſubject to the Controul and Direction of any fix'd *plaſtick Particle*, within the Verge of whoſe Activity they happen to move: notwithſtanding which, after they are once fix'd, they exert their own *plaſtick Powers*, and, in Conjunction with the firſt *plaſtick Particle*, govern the future Concretion, in ſuch Manner as to form a ſeemingly irregular Criſtal, tho' compoſed of two or more regular Criſtals. Thus in *Figure* the *fifth* and *ſixth* A and C ſeem to have attracted amongſt the ſtony Particles, two plaſtick Particles, which afterwards exerting their own Powers, form the additional Criſtals B and D.

There are many Phænomena obſervable in theſe Criſtals, which, at preſent, I may paſs over, as leſs relating

lating to the Affair of *Metals*; wherefore I ſhall only add, that theſe criſtalline Concretions exert a ſtrong Attraction on many metallick Subſtances. As in *Fig* 7. A the Spar has attracted the three Portions of Lead B. and in *Fig.* 8. the Criſtals C have attracted the Copper D, and are attracted by the Lead E.

The *ſulphureo-ſaline* Particles, with which, as I obſerved, the Waters are frequently ſaturated, are found to be either of a vitriolick or an arſenical Nature: The firſt conſtantly, if pure, concreting into white *Cubes* reſembling Grains of Silver, while the arſenical Sulphur concretes into yellow Cubes like Grains of pure Gold. Both theſe are by the Miners term'd *Mundick*.

Theſe *ſulphureo-ſaline* Subſtances ſeem directed in their Concretions by a plaſtick Particle, in the ſame Manner as the Criſtals above-mention'd; and, like them, upon the ſame Principles, are found ſimple or compound. In their Sides you may obſerve the Concretion forms it ſelf like Threads, which in three Sides run in different Directions, but are always ſimilar in the oppoſite Sides.

Fig. 9. ſhews one of theſe Cubes, A the parallel Threads.

Fig. 10. ſhews another of theſe Cubes, from whoſe Sides ariſe ſmall Segments of Cubes C.

But this *plaſtick Power* ſeems to be weaken'd or deſtroy'd, in Proportion, as this ſulphureous Matter is more or leſs intangled with metallick Subſtances.

Thus in *Fig.* 11. the plaſtick Particle ſeems for a while to have exerted its Power in the uſual Manner, till the advening Matter grew intangled with a ſmall Quantity of Copper, after which it ſeems only to have exerted its *attractive* but not its *plaſtick Power*.

And

And in *Fig.* 12. the white *Mundick* being infected with Iron, ſeems ſo far from being affected by a *plaſtick Power*, that it concreted in the Form of Icicles from the Fluid which tranſuded thro' the Top of the Mine.

Fig. 13. repreſents ſome ſmall Cubes of white or vitriolick *Mundick*.

But to return to the Mines: They are found to contain Tin, Lead, Copper, Iron, and a pſeudometallick Subſtance, by the Miners term'd *Gliſt*: For the Particulars of all which, as they would vaſtly ſwell the Bulk of one Letter, I muſt refer you to my next.

I am,

With the utmoſt Reſpect,

Your obliged Friend,

Fran. Nicholls.

VI. *Obſervationes Aſtronomicæ habitæ* Ulyſſipone, *Anno* 1726. *à Rev. P.* Joh. Baptiſta Carbone, *Soc. Jeſ. Communicante* Iſaaco Sequeyra Samuda, *M. D. R. S. S. & Coll. Med. Lond. Lic.*

Immerſiones & Emerſiones Primi Jovis Satellitis à Maii 23°. *ad finem Anni* 1726.

			h	′	″
Maii	23.	Debilitatio lucis -	15	24	0
		Totalis Immerſio -	15	24	40
Jul.	1.	Debilitatio lucis -	13	46	29
		Immerſio totalis -	13	47	47
	8.	Debilitatio lucis -	15	40	30
		Immerſio totalis -	15	41	40
	17.	Debilitatio lucis -	12	0	45
		Totalis Immerſio -	12	1	52
Aug.	9.	Immerſio totalis -	12	13	30
	16.	Attenuatio luminis -	14	7	33
		Totalis Obſcuratio -	14	8	46
	23.	Attenuatio lucis -	16	3	10
		Immerſio totalis -	16	4	23
	25.	Debilitatio luminis -	10	31	40
		Totalis Obſcuratio -	10	32	57
Sept.	1.	Attenuatio lucis -	12	27	57
		Totalis Immerſio -	12	29	29
	10.	Debilitatio lucis -	8	53	47
		Totalis Obſcuratio -	8	54	54
	15.	Immerſio in Umbram -	16	21	32
	17.	Attenuatio luminis -	10	50	12
		Totalis Immerſio -	10	51	39
	24.	Debilitatio lucis -	12	46	38
		Totalis Obſcuratio. *Dubia.*	12	47	45
Octob.	10.	Immerſio totalis. *Nonnihil dubia.*	11	8	34

Emer-

	Emerſiones.	H.	′	″
Octob. 26.	Initium Emerſionis -	11	39	41
	Integra luminis reſtauratio	11	41	2
Octob. 28.	Emerſio ab Umbra -	6	8	52
	Emerſio à Penumbra -	6	10	4
Nov. 4.	Initium Emerſionis -	8	6	3
	Totalis Emerſio -	8	7	14
11.	Initium Emerſionis -	9	57	39
	Integra lucis reſtauratio -	9	58	49
20.	Emerſio ab Umbra -	0	20	19
	Emerſio à Penumbra	6	21	29
Dec. 4.	Emerſio ab Umbra. *Dubia.*	10	5	18
	Totalis reſtauratio luminis	10	6	34

Continuatio obſervationum ad elevationem Poli Ulyſſipone *inquirendam.*

Die 3. Maii.

	Gr.	′	″
Altitudo Meridiana limbi ſuper. Solis *Quadrante Murali quinque pedum*	67	5	25
Correctio additiva Quadrantis -		10	15
Altitudo apparens ejuſdem limbi -	67	15	40
Refractio - - - -			23
Altitudo correcta, & vera limbi ſuperioris - - -	67	14	17
Semidiameter Solis apparens - -		15	56
Altitudo vera centri - -	66	58	21
Declinatio Borealis - -	15	40	36
Altitudo Æquatoris - -	51	17	45
Elevatio Poli. - - - -	38	42	15

Die

	Gr.	′	″
Die 22. *Junii.*			
Altitudo Meridiana limbi ſuper. Solis; *Quadrante aſtronomico trium pedum*	75	5	20
Correctio Quadrantis ſubtrahenda		2	37
Altitudo apparens prædicti limbi -	75	2	43
Refractio - - -			15
Altitudo vera limbi ſuperioris -	75	2	28
Semidiameter Solis apparens -		15	53
Altitudo vera centri Solis - -	74	46	35
Ejuſdem Declinatio Bor. - -	23	28	50
Altitudo Æquatoris - - -	51	17	45
Elevatio Poli	38	42	15
Die 11. *Auguſti.*			
Altitudo Meridiana Lucidæ Lyræ, *Quadrante Murali quinque pedum* -	89	40	15
Correctio Quadrantis, addit. -		10	15
Altitudo vera ſideris - -	89	50	30
Declinatio Borealis, ex tabulis Cl. V. Joannis Flamſteadi - - -	38	32	55
Altitudo Æquatoris - -	51	17	35
Elevatio Poli - - -	38	42	25
Die 14. *Auguſti.*			
Altitudo Meridiana limbi ſup. Solis, *eodem Quadrante Murali* -	65	49	13
Correctio Quadr. add. - -		10	15
Altitudo apparens ejuſdem limbi -	65	59	28
Refractio			24
Altitudo vera limbi ſnperioris -	65	59	4
Semidiameter apparens Solis -		15	53
Altitudo vera centri- - -	65	43	11
Declinatio Bor. - - -	14	25	30
Altitudo Æquatoris - - -	51	17	41
Elevatio Poli - - -	38	42	19

Die

Die 25. Septembris.

	Gr.	′	″
Altitudo Meridiana limbi ſup. Solis, *Quadrante aſtronomico trium pedum*	50	47	11
Correctio Quadrantis ſubtr.		2	37
Altitudo apparens ejuſdem limbi	50	44	34
Refractio			44
Altitudo apparens correcta	50	43	50
Parallaxis			2
Altitudo vera limbi ſuperioris	50	43	52
Semidiameter Solis apparens		16	4
Altitudo vera centri Solis	50	27	48
Ejuſdem Declinatio Auſtralis		50	12
Altitudo Æquatoris	51	18	0
Elevatio Poli	38	42	0

Die 27. Octobris.

	Gr.	′	″
Altitudo Meridiana limbi ſup. Solis, *Sextante*	38	40	15
Correctio Inſtrumenti additiva		6	5
Altitudo apparens ejuſdem limbi	38	46	20
Refractio		1	6
Altitudo limbi ſuperioris corr.	38	45	14
Parallaxis			3
Altitudo vera ejuſdem limbi	38	45	17
Semidiameter ſolis apparens		16	12
Altitudo vera centri	38	29	5
Ejuſdem Declinatio Auſtralis	12	48	34
Altitudo Æquatoris	51	17	39
Elevatio Poli	38	42	21

Eodem die 27. Octobris.

Altitudo Meridiana *Fomahantis Aquarii* seu Lucidæ in ore Piſcis Auſtrini, *eodem Sextante*	Gr. 20	′ 9	″ 55
Correctio Inſtrumenti additiva		6	5
Altitudo apparens	20	16	0
Refractio		2	26
Altitudo vera ſideris	20	13	34
Declinatio Auſtralis ex *De La Hire*	31	3	59
Altitudo Æquatoris	51	17	33
Elevatio Poli	38	42	17

Ex his, aliiſque Obſervationibus inferre hactenus licet, *Ulyſſiponenſem Latitudinem* in Obſervatorio Regii Palatii, non excedere grad. 38° 43′, nec minorem eſſe grad. 38° 42′. Cæterùm, ad minuta ſecunda quod attinet, nihil audeo certi definire; neque enim hujuſmodi Obſervationes ad id præſtandum ſatis eſſe arbitror; quæ ſcilicet, vel Inſtrumentorum defectu, utcunque exiguo, vel refractionum inconſtantiâ ab aëris inæqualitate, vel Aſtronomorum diſſidio in Declinationibus ſiderum ſtatuendis, vix aliquando contingat, ut omni prorſus careant errore; nec ſi aliquando careant, id ullo poterit indicio deprehendi. Ex prædictis tamen Obſervationibus, non imprudenter ſtatui poteſt *latitudo Obſervatorii Regii in aulâ* 38° 42′ 20″, *noſtri verò Collegii D. Antonii Magni*, 38° 42′ 30″. Quamquam hæc ipſa ſecunda prudens neglexi in nonnullis ſupputationibus, in quibus Elevatione Poli fuit utendum; tum quòd nullum neglecta, aut nullius momenti errorem inducere poſſent; tum etiam quòd aliarum Obſervationum indi-

cio

cio pauciora, aut penè nulla ea eſſe viderentur. Mox tamen novis, variiſque Obſervationibus inſtituendis, ac præſertim magni Gnomonis ope, Auguſtiſſimi noſtri Regis juſſu erigendi, fore ſpero, ut ſecundorum numerum congruentiùs aſſequamur.

Quas hîc obſervavi Solis, Lunæque Eclipſes, obſervavit R. P. *Dominicus Capaſſus, Conimbricæ*, quò, à menſe Junio anni proximè ſuperioris 1726. balneorum causâ ſeceſſerat; ibique nonnullas etiam inſtituit Obſervationes circa Poli Elevationem; ac circa Intimi Satellitis Immerſiones, atque Emerſiones; quas omnes ſeorſim exhibebit, unà cum reliquis ab ipſo mox habendis in variis hujuſce Regni locis ad Boream ſitis. Deinceps in aliis etiam opportunius inſtituentur.

VII. *Some Obſervations made in the Diſſection of three Subjects*; *by Mr.* Ranby, *F. R. S.*

THE *Firſt*, a Man aged 70 Years, who died of a Suppreſſion of Urine, occaſion'd by a Stone ſtopping in the *Urethra*, juſt within the *Glans*, of the Bigneſs of a Horſe-Bean. This Appearance, with the Symptoms that had attended this miſerable Man, gave me Reaſon to expect ſomething remarkable in the urinary Paſſages. The *Ureters* and *Pelvis* were very much diſtended; which is common where great Numbers of Stones have deſcended down them, from the Kidneys to the Bladder. The Bladder contained about *ſixty Stones*, the largeſt of which was about the Size of a Walnut, the others ſmaller; and juſt within the Neck, was a *hard Tumour*, as big as a Nutmeg, which almoſt

almoſt cloſed the Orifice: and indeed the Situation of this Tumour was ſuch, that it not only made the paſſing the *Catheter* very difficult, and hinder'd our feeling the Stones, by directing the Inſtrument upwards; but likewiſe would alone produce the Symptoms of the Stone in the Bladder, by obſtructing the free Diſcharge of Urine through the *Urethra*; the inner Membrane of which appeared as if lacerated in ſeveral Places, and the Tube fill'd with a glutinous Matter tinged with Blood. On the back Part of the *Veſiculæ Seminales*, near the *Proſtata*, were ſeveral *Stones*, as large as Peas, which cloſely adhered to the adjacent Membranes.

The *Second*, a Boy aged 10 Years, kill'd by a Blow on the Skull; whoſe Spleen weigh'd two Pounds, and poſſeſſed almoſt all the left Side of the abdominal Cavity. The Bladder, when diſtended to its greateſt Capacity, would not contain an *Ounce*.

The *Third*, a Man aged 25, who died of a Pocky Hectick, and ſome Days before complained of a painful Swelling in the Teſticle, which he ſaid came the Night before. I examin'd it, and found it to be a *Hernia Aquoſa*, and would have punctur'd it, if I had not felt (beſides the Water) a hard Body, which I could by no Means reduce. In a few Days he died, which gave me an Opportunity of being ſatisfied. Opening the *Scrotum*, and ſeparating the common Membranes to the *Proceſſus Vaginalis*, it contained about four Ounces of Water, beſides a great Part of the *Omentum*; ſome Portion of which adhered to the Bottom of the Cavity, and the *Albuginea* that immediately covers the Teſticle.

F I N I S.

Jos: Banks

Numb. 402.

PHILOSOPHICAL TRANSACTIONS

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I. *Ob-*

I. *Obſervations of the* Eclipſes *of* Jupiter's Satellites, *from* 1700, *to the Year* 1727. *By the Reverend* W. Derham, *M. A. Canon of* Windſor *and F. R. S. Communicated by Sr.* Hans Sloane, *Bart. Preſident of the* College *of* Phyſicians *and* Royal Society, *&c.*

Eclipſes Primi Satellitis.

Dies Menſis.	Tempus æquale.	Tempus apparens.	Per Tab. Flamſt. & Caſſini.	Qualis Eclipſis.	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	Min. Sec.		Grad.	
		Anno Domini 1700.				
Aug. 13	10. 59. 4	10. 57. 10	59. Fl.	Em.	♒ 15	Teleſcopio 6. pedali.
Dec. 1	— — —	5. 1. 8 — 1. 38	$4^h.\ 55'\frac{1}{2}$ C. 4. 58 Fl.	E.	♒ 10	Teleſc. 16. pedali.
						Omnes ſequentes Eclipſes Tubo 16. pedali obſervatæ fuere, niſi cum aliter notatur.
		Anno Domini 1701.				
Jun. 15	— — —	13. 23. 0 — 24. 50	13. 21 Fl. 13. 26 C.	Im.	♒ 27	
Jul. 8	— — —	13. 30. 0	13. 28 Fl. 13. 34 C.	I.	♒ 29	
Oct. 12	— — —	5. 54. 9 — 54. 19 — 54. 49	5. 59 C.	E.	♓ 8	Bona Obſervatio.
— 19	— — —	7. 48. 57 — 49. 47	7. 55 C.	E.	♓ 9	Bona.
Dec. 20	— — —	6. 25. 0	6. 28 Fl.	E.	♓ 14	Dubia.
		Anno Domini 1702.				
Oct. 15	— — —	9. 22. 0 — 22. 15 — 22. 45	9. 23 Fl. 9. 26 C.	E.	♈ 11 $\frac{1}{2}$	Optima.
— 24	— — —	5. 44. 57 — 45. 21 — 45. 42	5. 47 Fl. 5. 50 C.	E.	♈ 12	Optima.
Dec. 9	— — —	5. 59. 0	6. 5 Fl.	E.	♈ 16 $\frac{1}{2}$	Aer nebuloſus.

Dies Menſis.	Tempus æquale.	Tempus apparens.	Per Tab. Flamſt. & Caſſini.	Eclipſis qualis.	Locus Jovis.	
	H. M. S.	H. M. S.	H. Min.		Grad.	
		Anno Domini 1703.				
Aug. 8	— — —	15. 21. 30	15. 18 F.	I.	♋ 8½	Dubia ob Nebulas.
—— 24	— — —	13. 43. 6 — 43. 35	13. 38 F. 13. 45 C.	I.	♌ 10	Bona.
Sep. 2	— — —	10. 8. 20	10. 4 F.	I.	♌ 11	Nebuloſum Cœlum.
Nov. 28	5. 37. 40	5. 44. 52	5. 43 F.	E.	♌ 19	Non mala.
		Anno Domini 1704.				
Aug. 26	17. 7. 43	17. 9. 53	17. 8 F.	I.	♊ 12¾	Nimia lux, ſed non mala.
—— 28	11. 36. 16	11. 38. 18 — 39. 3	11. 37 F.	I.	♊ 13	Bona.
Sept. 4	13. 29. 14	13. 33. 5 — 34. 25	13. 23 F.	I.	♊ 13½	Bona.
Oct. 6	10. 2. 40	10. 16. 40 — 17. 10	10. 13 F.	I.	♊ 16	Nebuloſa, ſed non mala.
Nov. 3	17. 39. 54	17. 54. 55	17. 48 F.	I.	♊ 18¾	Nebuloſum.
Dec. 9		5. 31. 00		E.	♊ 21¾	Dubia.
—— 23	9. 14. 43	9. 9. 17	9. 10. C.	E.	♊ 23	Bona.
		Anno Domini 1705.				
Mart. 2	— — —	9. 46. 3 — 46. 40 — 47. 0	9. 47 F.	E.	♊ 29	Bona.
—— 25	10. 9. 58 10. 11. 5	10. 7. 18 — 7. 52 — 8. 25	10. 10 F.	E.	♋ 0½	Bona.
Sept. 7	16. 39. 15 — 40. 15	16. 45. 26 — 46. 26	16. 52 F.	I.	♋ 15	Bona.
Oct. 30	18. 54. 50 — 55. 20	19. 10. 19 — 10. 49	19. 10 C.	I.	♋ 19	Nimia lux, ideo dubia.
Nov. 22	19. 4. 50	19. 13. 59		I.	♋ 21	
Dec. 15	19. 15. 0	19. 13. 20	19. 11 F.	I.	♋ 22¼	Dominus Gray hanc immerſionem Cantuariæ 19h. 15′ obſervavit.
		Anno Domini 1706.				
Mart. 7	— — —	7. 27. 0	7. 26 F.	E.	♋ 29½	Emerſum inveni.
—— 30	7. 46. 40 — 47. 37	7. 45. 25 — 46. 20	7. 48 F.	E.	♌ 3	Bona.
Apr. 29	9. 59. 2	10. 3. 5	10. 3 F.	E.	♌ 3¾	Nebuloſum.

Dies

Dies Menſis.	Tempus æquale.	Tempus apparens.	Per Tab. Flamſteed. & Caſſini.	Qualis Eclipſ.	Locus Jovis.	
	H. M. S.	H. M. S.	H. Min.		Grad.	
		Anno Domini 1707				
Feb. 15	10. 17. 52	10. 4. 43	10. 3 F.	E.	♌ 26¼	
— 24	6. 39. 52	6. 28. 20	6. 28 F.	E.	♌ 27¼	Bona.
	— 41. 32	— 30. 0				
Mar. 26	8. 47. 53	8. 45. 36	8. 46 F.	E.	♌ 29¾	Ventus fortis tubum motitavit.
	— 48. 6	— 43. 49				
Maii 11	9. 14. 43	9. 18. 43	9. 20 F.	E.	♍ 3½	Bona.
	— 16. 7	— 20. 7				
		Anno Domini 1708.				
Jan. 31		17. 35. 0		I.	♍ 23½	Immerſus fuit ante.
		Anno Domini 1709.				
Maii 18	9. 7. 26	9. 10. 47	9. 18 F.	E.	♎ 1¾	Non mala.
	— 8. 26	— 11. 47	8. 59 C.			
Jun. 10	9. 22. 21	9. 21. 9	9. 27 F.	E.	♎ 3½	Bona.
	— 24. 0	— 23. 8	9. 12 C.			
		Anno Domini 1710.				
Maii 14	10. 6. 57	10. 10. 26	10. 14 F.	E.	♏ 26	
Jul. 15	— — —	8. 46. 0	8. 49 F.	E.	♐ 2.	Immerſum vidi.
Aug. 23	7. 22. 2	7. 22. 50	7. 28 C.	E.	♐ 5	Non mala.
		Anno Domini 1711.				
Aug. 19	— — —	8. 23. 40	8. 32 F.	E.	♑ 4	Bona.
		— 24. 30	8. 30 C.			
		— 25. 00				
		Anno Domini 1713.				
Oct. 27	8. 10. 28	8. 26. 19	8. 35 F.	E.	♓ 14	Bona.
Dec. 28	7. 4. 18	6. 56. 30	7. 5 F.	E.	♓ 19¾	Bona.
			7. 1 C.			
		Anno Domini 1714.				
Oct. 23	9. 59. 0	10. 1. 1	10. 5 F.	E.	♈ 17	Dubia.
Nov. 1	— — —	6. 20. 0	6. 28 F.	E.	♈ 18	Nubilum ideo dubia licet tubo 34 pedali obſervationem feci.

Dies Menſis.	Tempus æquale.	Tempus apparens.	Per Tab. Flamſt. & Caſſini.	Qualis Eclipſ.	Locus Jovis.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1717.				
Feb. 1	6. 25. 3	6. 10. 15	6. 14 F.	E.	♋ 1	Bona.
	— 26. 18	— 11. 30	6. 14 C.			
	— 26. 48	— 12. 00				
— — 8	8. 21. 21	8. 7. 0	8. 9 F.	E.	♋ 1½	Bona.
	— 21. 51	— 7. 30	8. 12 C.			
	— 22. 21	— 8. 0				
		Anno Domini 1725.				
			Per Tabul. D. Bradley.			Sequentes Obſervationes Teleſcopio 12½ pedali optimo factæ fuere.
Nov. 27	— — —	9. 8. 36	9. 6. B.		♓ 22	Windeſoriæ, dubia.
		9. 0				
		Anno Domini 1726.				
Jan. 5	7. 40. 19	7. 29. 49	7. 31 B.	E.	♓ 25	Bona, Upminſtro.
	— 41. 0	— 30. 0				
	— 41. 30	— 31. 0				
Aug. 5	14. 50. 17	14. 46. 28	14. 47 B.	I.	♈ 15	Bona.
	— 51. 7	— 47. 18				
Sep. 15	7. 44. 17	7. 53. 10	7. 53 B.	I.	♈ 19	Bona.
	— 45. 37	— 54. 30				
	— 46. 7	— 55. 0				
Oct. 8	— — —	10. 25. 0	10. 7 B.	E.	♈ 21	Obſervatio incerta is propter vicinitatem.
— — 17	— — —	6. 46. 30	6. 46 B.	E.	♈ 21½	☿.
Dec. 2	— — —	7. 2. 0	7. 5 B.	E.	♈ 25	Windeſoriæ, dubia.
		— 3. 0				
— — 25	7. 12. 57	7. 7. 0	7. 9 B.	E.	♈ 28	Upminſtri bona.
	— 13. 57	— 8. 0				

Obſervationes Eclipſium Secundi Satellitis Jovis.

		Anno Domini 1700.				
Oct. 27	— — —	8. 24. 0	8. 23	E.	♒ 6¾	Sequentes Obſervationes Teleſcop. 16 pedali factæ fuere. Dubia propter vapores. D. Flamſteedii Miniſter obſervavit circa 8h. 16′. p. m.

Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteedii.	Qualis Eclipſ.	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1701.				
Jun. 29	--- --- ---	10. 50. 0	10. 52	I.	♒ 28 ½	Dubia ob vapores.
Jul. 31	Inter	9. 43. 0 10. 3. 0	10. 33	I.	♓ 1	Nubiloſum.
Oct. 21	--- --- ---	7. 39. 35 — 40. 0 — 41. 0	7. 51	E.	♓ 8 ¾	Bona.
---- 28	--- --- ---	10. 18. 2 — 20. 0	10. 29	E.	♓ 9	Bona.
Nov. 22	--- --- ---	7. 26. 18 — 27. 0	7. 34	E.	♓ 11 ½	Bona.
		Anno Domini 1702.				
Aug. 26	--- --- ---	9. 46. 0	10. 17	I.		Immerſum inveni.
Sep. 9	--- --- ---	15. 0. 51 — 1. 51	15. 35	I.	♈ 8	Bona.
Oct. 15	--- --- ---	7. 5. 21 — 6. 22 — 7. 22	7. 30	E.	♈ 11 ½	Bona.
---- 22	--- --- ---	9. 40. 38 — 41. 0 — 42. 13	10. 8	E.	♈ 12	Bona.
		Anno Domini 1703.				
Aug. 20	--- --- ---	9. 50. 0	10. 11	I.	♉ 9 ¾	Immerſum inveni.
Oct. 5	--- --- ---	15. 3. 0	15. 19	I.	♉ 24	Immerſus ante.
Dec. 19	8. 41. 9	8. 38. 7	9. 6	E.	♉ 20 ½	Bona.
		Anno Domini 1704.				
Aug. 20	12. 15. 29	12. 15. 34	12. 32	I.	♊ 12	Bona.
Oct. 5	--- --- ---	17. 10. 44 — 11. 14	17. 32	I.	♊ 16	Bona.
---- 16	--- --- ---	9. 2. 19	9. 26	I.	♊ 10	Haud mala.

Dies

Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteedii.	Qualis Eclipſ.	Locus Jovis Helioc.	
	H. M. S	H. M. S	H. M.		Grad.	
		Anno Domini 1705.				
Jan. 13	— — —	7. 47. 0	8. 14	E.	♊ 24¼	Emerſio ante.
—— 20	10. 30. 23 — 31. 14	10. 16. 9 — 17. 0	10. 50	E.	♊ 25½	Bona.
Feb. 14	7. 45. 31	7. 32. 0	8. 1	E.	♊ 27⅓	Vaporoſus aer: dub.
Mar. 25	10. 15. 10 — 16. 5	10. 12. 30 — 13. 25	10. 36	E.	♋ 0	Incerta propter vicinitatem primi Sat.
Apr. 26	— — —	10. 4. 0	10. 29	E.	♋ 3⅓	VaporoſusHorizon: dub.
Sep. 29	16. 26. 11 — 26. 26	16. 39. 11 — 39. 26	16. 48	I.	♋ 16½	Bona.
Oct. 31	15. 55. 20 — 56. 26	16. 10. 52 — 11. 49	16. 27	I.	♋ 19	Bona.
Dec. 20	9. 49. 30 — 50. 10	9. 45. 36 — 46. 16	10. 11	I.	♋ 23	Bona.
		Anno Domini 1706.				
Ap. 20	— — —	8. 58. 30	9 31	E.	♌ 3	Fuerat emerſus.
		Anno Domini 1707.				
Mar. 13	— — —	7. 59. 0	8. 18	E.	♌ 28¾	Emerſit ante.
—— 20	10. 28. 14	10. 23. 54	11. 5	E.	♌ 29	Dubia.
Ap. 14	7. 33. 17	7. 35. 39	8. 9	E.	♍ 0½	Dub. propter nim. lucem.
—— 21	10. 9. 33 — 10. 59	10. 12. 51 — 14. 17	10. 45	E.	♍ 1¾	Bona.
		Anno Domini 1710.				
Mar. 4	— — —	17. 5. 0	— — —	I.	♍ 21½	Nubilum, & incerta.
		Anno Domini 1711.				
Jul. 15	— — —	9. 16. 0	9. 6	E.	♍ 1½	Nebuloſum & dub.
		Anno Domini 1712.				
Oct. 12	7. 35. 6 — — —	7. 50. 30 — 51. 15 — 52. 30	7. 58	E.	♎ 10	Bona.
		Anno Domini 1714.				
Nov. 1	— — —	6. 34. 7	6. 55	E.	♈. 18	Incerta ob nubes.
Dec. 3	— — —	6. 14. 26 — 15. 0	6. 34	E.	♈. 21¼	Bona.
		Anno Domini 1716.				
Dec. 22	— — —	4. 55. 0	— — —	E.	♊ 27½	Emerſio ante fuit.

Dies

Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſt. & Molyn.	Qualis Eclipſ.	Locus Jovis Helioc.	
	H. M. S	H. M. S	H. M		Grad.	
		Anno Domini 1717.				
Jan. 30	— — —	7. 24. 0	7. 46	E.	♋ 1	Emerſum inveni.
		Anno Domini 1725.				
Oct. 30	— — —	9. 16. 23 — 17. 0 — 17. 18	8. 46 M.	E.	♓ 19	Bona obſervatio Teleſcopio 12 ½ peſdali opt.
		Anno Domini 1726.				
Aug. 28	8. 52. 25 — 53. 55	8. 54. 52 — 56. 22	8. 58 M.	I.	♈ 17	Bona.
Oct. 17	— — —	6. 12. 0	5. 53 M.	E.	♈ 21 ½	Dubia.
Nov. 25	— — —	8. 27. 15 — 28. 0	8. 16	E.	♈ 25	Non mala, Windeſoriæ.

Obſervationes Eclipſium Tertii Satellitis Jovis.

		Anno Domini 1700.				
Oct. 19	— — —	6. 23. 15	6. 13	I.	♒ 6	
		Anno Domini 1701.				
Jul. 11	— — —	10. 18. 0	10. 11	I.	— —	Dubia.
Sep. 28	— — —	10. 25. 27 — 26. 0 — 26. 42	10. 12	E.	♐ 6 ½	Bona.
		Anno Domini 1702.				
Jul. 26	— — —	10. 51. 13	10. 33	E.	♈. 4	Dubia.
Aug. 2	Inter	11. 55. 0 12. 5. 0	11. 35	I.	♈. 5	Nubes interrumpebant.

Dies

Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteedii.	Qualis Eclipſis.	Locus Jovis Helioc.	
	H. M. S	H. M. S	H. M.		Grad.	
		Anno Domini 1703.				
Jul. 12	— — —	14. 46. 0	15. 32	I.	♂ 6	Fuit Immerſus.
Aug. 24	— — —	13. 43. 20 — 44. 20 — 44. 51	13. 4	I.	♂ 10	Optima obſervatio Immerſionis : at ſomno invadente, nimis ſerò
		16. 6. 36	15. 40	E.		Emerſionem vidi.
Sep. 22	— — —	8. 9. 53	7. 50	E.	♂ 12 ¾	
—— 29	— — —	9. 57. 44 — 58. 5	9. 17	I.	♂ 13	Immerſio accuratè viſa, ſed Emerſio dubia propter vicinitatem Jovis.
		12. 8. 17	11. 52	E.		
Oct. 6	13. 46. 13 — 47. 33 — 48. 33	14. 0. 30 — 1. 50 — 2. 50	13. 19	I.	♂ 14	Bona.

In Obſervationibus Aug. 24, Sep. 22, 29, & Oct. 6. notandum eſt, Latitudinem tertii Satellitis majorem fuiſſe quam C. Flamsteedius, aut Cassinus conjectarunt. Nam iſte Satelles uſq; ad extremum Poli Jovis marginem evagatus eſt, & (antequam in ejus umbram prorſus immerſus eſt) diu in ejus penumbrâ latuit: & in eadem umbrâ non ultra duas horas permanſiſſe autumo, quamvis poſt Obſervationes Aug. 24, & Sep. 29, moram diuturniorem fuiſſe videatur. Sed in priore obſervatione, veram non vidi Emerſionem : & in poſteriore cum ✱ ☉ 4 fuerit) Emerſio Jovis limbo tam propinqua fuit, ut difficile fuerit eam cum Teleſcopio 16 pedali vere obſervare.

		Anno Domini 1704				
Sep. 14	Inter	16. 58. 0 17. 10. 0	16. 32	E.	♊ 14 ¼	Nubilum cœlum.
		10. 28. 40	9. 48	I.	♊ 17	Bona.
Oct. 20	10. 14. 34	— 30. 33				
Nov. 3	18. 12. 16	18. 27. 17	17. 45	I.	♊ 18 ¾	Bona, licet aer nebulos.
		Anno Domini 1705.				
Jan. 14	10. 7. 5 — 9. 4	9. 53. 57 — 55. 56	— — —	I.	♊ 24 ¾	Bona.
	— — —	9. 58. 7				
Feb. 26	10. 9. 32	— 58. 42	— — —	I.	♊ 28 ½	Bona.
Oct. 6	Inter	17. 18. 0 — 27. 0	— — —	E.	♋ 17	
Nov. 25	— — —	7. 41. 0	— — —	I.	♋ 21	Incerta per Nebulas denſas.

Dies

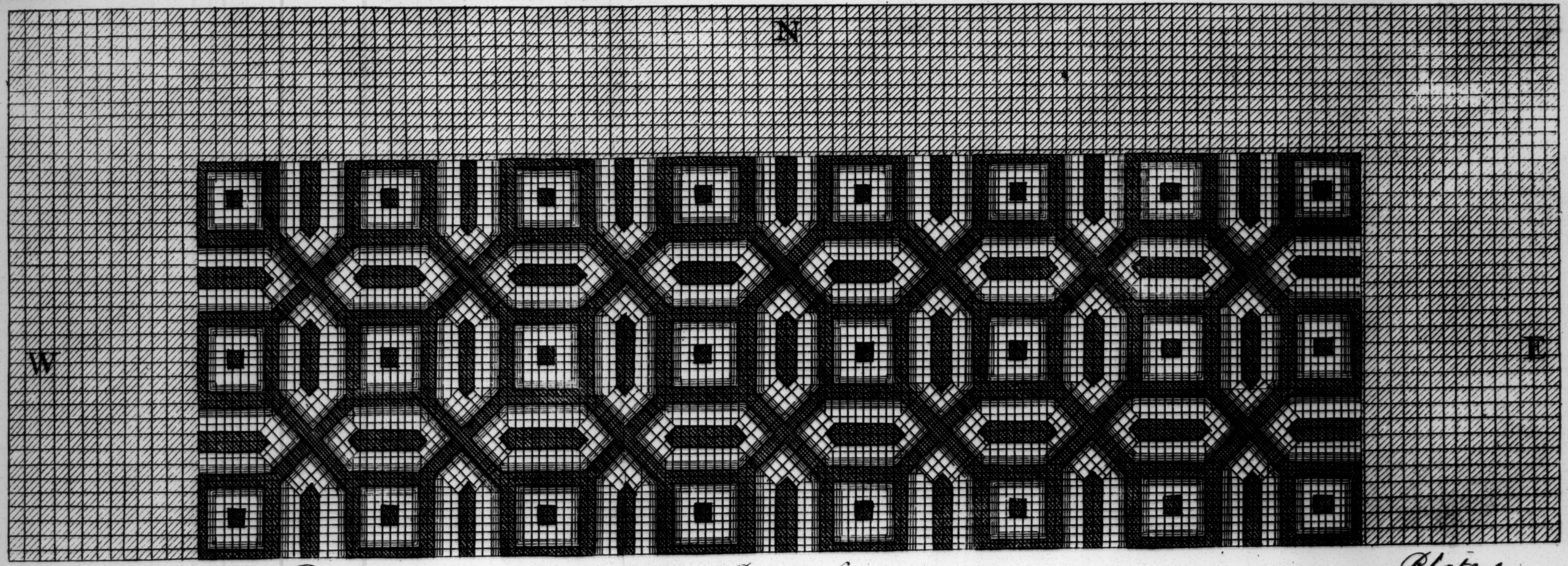
Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteedii.	Qualis Eclipſ.	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1706.				
Mar. 13	— — —	8. 52. 27 — 54. 0	7. 36	E.	♌ 0	Bona.
— 20	— — —	9. 24. 0	9. 3	I.	♌ 0 ½	Dubia ob ♃ vicinitatem.
Nov. 4	— — —	16. 59. 27	16. 50	I.	♌ 0 ¾	Non mala.
		Anno Domini 1707.				
Maii 17	— — —	8. 17. 0	— — —	E.	♍ 3 ¼	Emerſum inveni.
Nov. 26	18. 41. 37	18. 49. 15	18. 41	E.	♍ 18 ½	
Dec. 3	19. 19. 38 — 20. 46	19. 24. 1 — 25. 9	19. 25	I.	♍ 19	Bona.
		Anno Domini 1709.				
Feb. 5	17. 35. 32 — 37. 4	17. 21. 0 — 22. 32	17. 26	I.	♎ 21 ½	
		Anno Domini 1711.				
Jul. 14	— — —	9. 45. 0	9. 41	E.	♒ 2	Dubia.
		Anno Domini 1712.				
Sep. 16	— — —	6. 15. 0 — 20. 0	6. 28	I.		Dubia ob nimiam lucem.
		9. 50. 3 — 52. 0 — 52. 30	10. 5	E.	♒ 7 ¾	Bona.
Oct. 29	— — —	6. 26. 30 — 28. 0	6. 45	I.	♒ 11 ½	Bona.
		Anno Domini 1713.				
Dec. 26	6. 38. 30	6. 32. 0	6. 46	E.	♓ 19 ½	Ventus dubiam reddidit.
		Anno Domini 1714.				
Sep. 17	8. 26. 56 — 28. 56	8. 36. 9 — 38. 9	8. 40	I.	♈ 13 ¾	Bona, Tubo 34 pedali.
Oct. 23	7. 22. 0 — 23. 0 — 25. 0	7. 24. 0 — 25. 0 — 27. 0	7. 38	E.	♈ 17	Bona, Tubo 16 pedali.
		Anno Domini 1716.				
Dec. 5	— — —	— — —	7. 37	E.	— —	Hic Satelles magnam habet Latitudinem, & ni fallor, nullam paſſa eſt Eclipſin hâc nocte.

Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteedii.	Qualis Eclipſ.	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1717.				
Jan. 17	— — —	5. 52. 23 — 53. 45	— —	I.	♋ 0	Bona.
		8. 48. 38		E.		Mala.
		Anno Domini 1726.				
Jan. 5	6. 41. 10 — 42. 30	6. 30. 40 — 32. 0	— —	E.	♓ 25 ¼	Bona, Teleſcop. 12 pedali.
Dec. 15	7. 13. 29 — 14. 42	7. 12. 17 — 13. 30	7. 21 M.	E.	♈ 15 ¾	Bona.

Obſervationes Eclipſium Quarti Satellitis.

		Anno Domini 1701.				
Jun. 11	— — —	14. 20. 0	14. 35	I.	♒ 27	Immerſum inveni.
Sep. 3	— — —	13. 59. 0	14. 21	E.	♐ 4 ¾	Idem per nebulas.
		Anno Domini 1704.				
Sep. 30	— — —	9. 21. 42	9. 40	E.	♊ 15 ¾	Nebuloſum, ideo dubia.
Dec. 6	— — —	9. 52. 44	— — —	E.	♊ 21 ½	Dubia ob proximitatem Jovis, & parvitatem Satel.
		Anno Domini 1705.				
Feb. 11	8. 38. 5	8. 24. 8	8. 23	I.	♊ 27	Non mala.
		Anno Domini 1706.				
Mar. 20	9. 3. 2	8. 58. 40	9. 36	I.	♌ 0 ¼	Non mala.
Sep. 20	— — —	16. 24. 0	16. 17	I.	♌ 15	Immerſum inveni.
		Anno Domini 1712.				
Aug. 20	Inter.	8. 29. 0 — 39. 0	10. 11	I.	♒ 5	
		7. 43. 0 — 44. 0 — 45. 0	9. 20	E.	♒ 7	Bona.

Re-



Philo: Trans. N.o 402.

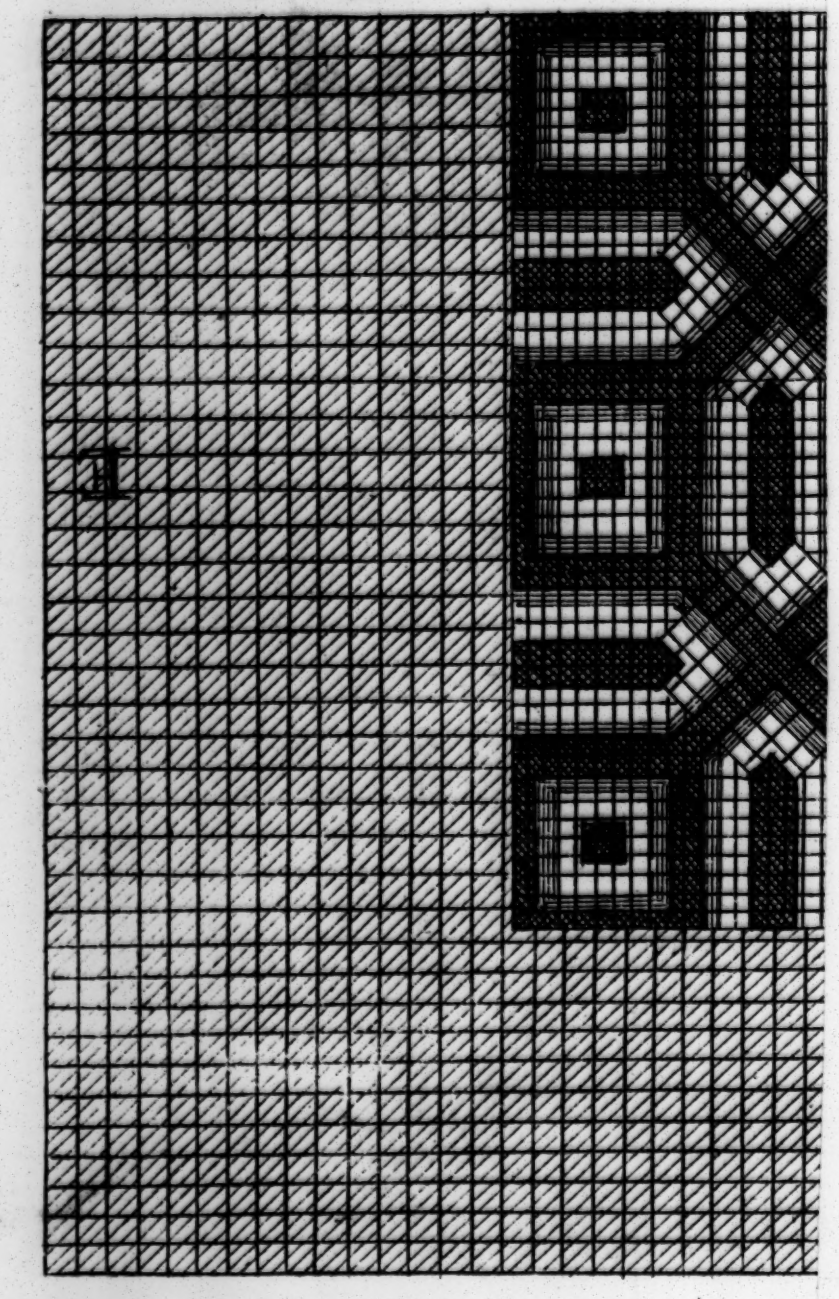
Plate 1.

Part of a Roman Mosaick Pavement found in Denton fields Febr. 1727/8

2 C 2

London Feb.ry 1792

Plate 1.



Remarks on the foregoing TABLES.

AS exact Tables to calculate the *Eclipſes* of the *Circumjovials*, would be of very great Service to find the *Longitude of Places*; ſo I have ſome Hopes that theſe Obſervations of ſome of them, in more Revolutions than one of *Jupiter* in his Orb, may be of Uſe to correct, or make ſuch Tables.

I wiſh that I could have made them more compleat (and in my younger, and more leiſurely Days, I endeavoured to do it, by riſing at unſeaſonable Hours, *&c.*) but beſides cloudy and bad Weather, one great Hindrance was the Want of Tables, to enable me to calculate the Eclipſes my ſelf, and the frequent Diſappointments of my Friends, that furniſhed me with Catalogues of them. And moreover, many times Company, and Buſineſs, and (to tell the Truth) ſometimes Forgetfulneſs, have hindred the Conſtancy of my Obſervations: But the greateſt Chaſms in them were cauſed by ſome dangerous Fits of Sickneſs, which ſo impared me, that I have not dared, ever ſince, to venture upon Obſervations at unſeaſonable Hours of the Night.

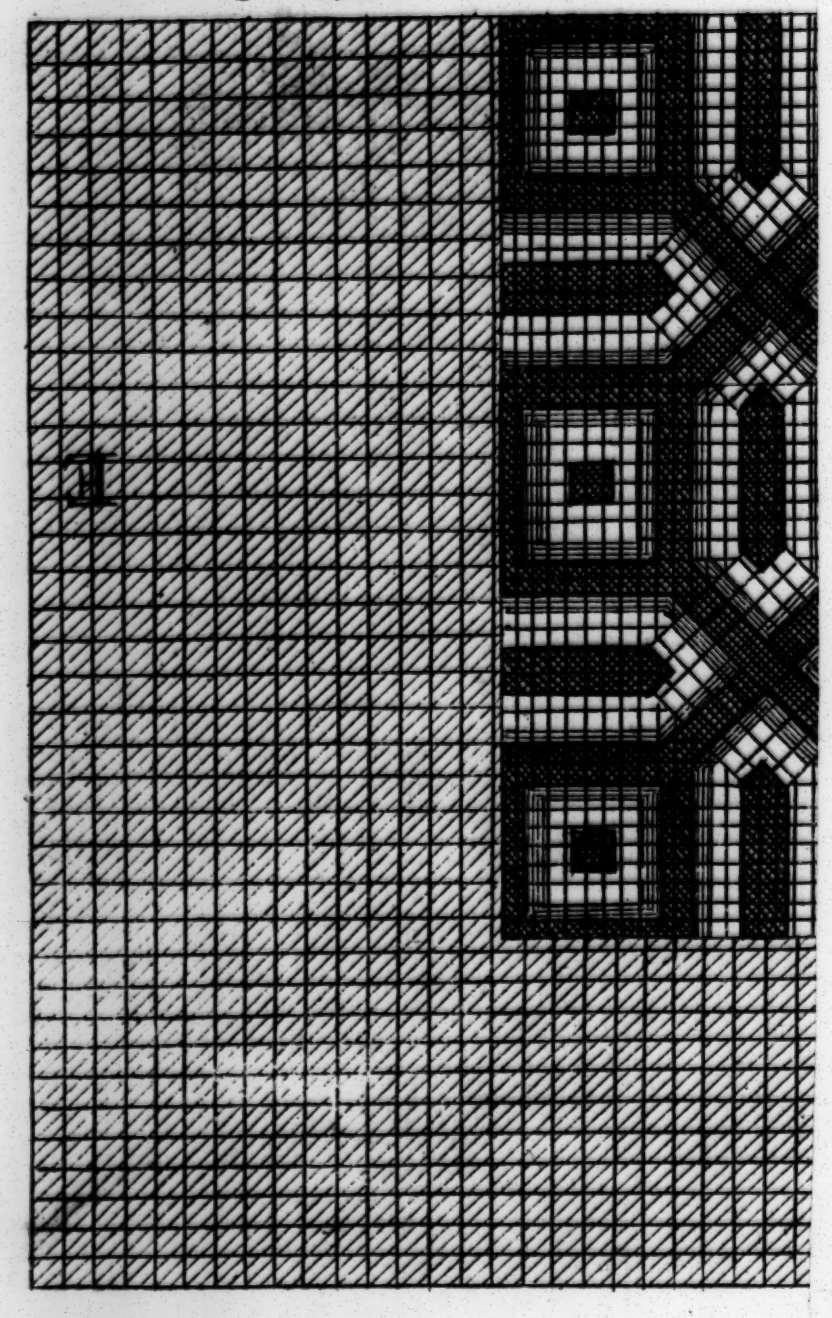
As to my Manner of obſerving, it was (for the moſt Part) with a 16 Foot Teleſcope, and afterwards with an excellent one (not inferior to it) of 12 $\frac{1}{2}$ Feet, that, at *Jupiter*'s Light, bears an Aperture of 2 $\frac{1}{2}$ Inches, and a Charge of about 2 Inches.

And as to the *Time*; I made uſe of an excellent and well-adjuſted *Clock*, corrected at Noon, by the Meri-

Lenton Fields. Feb.r 1792

8 9

Plate 1.



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And as to the *Time*; I made uſe of an excellent and well-adjuſted *Clock*, corrected at Noon, by the Meri-

dional Tranſits of the Sun, obſerved with the *Inſtrument* deſcribed in the *Philoſ. Tranſact.* N° 291, which ſhews the Noon-time to one or two Seconds. This Way ſome of my ſkilful Friends (particularly Mr. *Flamſteed*) ſuſpected to be fallacious, and not comparable to taking the Time by Altitudes of the Sun, or fix'd Stars. For a Trial therefore, I gave him a Challenge, of obſerving ſome Eclipſes that we agreed on; which when we compared, we found ſo nicely to agree, as to ſhew to a Second of Time, or very nearly ſo, the Difference of the Meridian of the *Obſervatory*, and that of *Upminſter*.

But becauſe the Credit of my Obſervations depends upon the Strictneſs of the Time, give me Leave to compare my *Inſtrument* with a *Quadrant*. And here I will preſume to affirm, that my *Inſtrument* is no more liable to Errors than a *Quadrant*. 1. It is leſs ſo, in regard of its Structure: For a little Error in the Diviſion of a Quadrant's Limb, or in fixing its Sights (whether Teleſcopick or Plain) ſpoils all. But no great Curioſity, or Niceneſs is required in my *Meridian-Inſtrument*. 2. In point of Obſervation, as much Care and Exactneſs is neceſſary to guard againſt Wind, and to take a true *Altitude* by the *Quadrant*, as is neceſſary to take a true *Azimuth* by my *Inſtrument*. For if we are ſure that the Inſtrument is exactly in the Meridian (the Manner of which is ſhewn in the fore-cited *Tranſaction*) we can be as ſure, and that to as few Moments of Time, as any the beſt Quadrant can ſhew it. And although I cannot ſay, that every Day, or every Week, I examined the Poſition of my Inſtrument, yet I did it ſo often, as to be ſatisfied, that

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not many, or great Errors, could be in my Obſervations.

The greateſt Part of the Eclipſes, that were the moſt accurately made, may eaſily be diſtinguiſhed by the two, or more Numbers of the Time of Obſervation: The firſt of which ſhews the Moment of the Beginning of the Eclipſe; the following, the Times when farther advanced: As in an Emerſion, the firſt Number ſhews the Time, when the Satellite appears like a ſmall obſcure Spot; the following Numbers, when brighter, or quite emerged out of *Jupiter*'s Shadow; and ſo contrariwiſe in an Immerſion.

But altho' this might have ſufficed, yet for greater Certainty and Satisfaction, I have noted which Obſervations were good, which doubtful, or bad: Even the latter of which may be of Uſe in ſome Caſes, where better are wanting.

The calculated Times of the Eclipſes I have inſerted, where I had them from others, or could calculate them my ſelf, as being of good Uſe to amend the Tables of Mr. *Flamſteed*, *Caſſini*, or others, taken Notice of in the Column on Purpoſe. And for the ſame Reaſon I thought good to add the Place of *Jupiter* alſo.

And laſtly, I thought it good to mention the Length and Power of the Teleſcope I uſed; as being, in ſome Meaſure, neceſſary in the comparing Obſervations of different Places; becauſe Obſervations may differ ſeveral Seconds, by the different Length and Goodneſs of the Teleſcope uſed; a long and good Teleſcope ſhewing the Satellite, when the Shadow of *Jupiter* doth but juſt touch it: Whereas a ſhort, or bad one, doth not ſhew it, until one half, or more, of the Satellite is enlightened. Which Difference is moſt remarkable in

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the Eclipſes of the two outermoſt Satellites, in their greateſt Latitudes; at which Times they go into, and come out of *Jupiter*'s Shadow, in an oblique and longer, not a direct and ſhorter Path: An Inſtance of which may be ſeen in the Obſervations of the Eclipſes of the Third Satellite in the Months of *Auguſt* and *September*, 1703.

II. *A Deſcription of a* Roman Pavement *found near* Grantham *in* Lincolnſhire, *with the Oeconomy of the* Roman *Times in this Part of* England, *communicated in a Letter to Dr.* Rutty, *R. S. Secr. by W.* Stukeley, *M. D. Col. Med. Lond. & R. S. Soc.*

SIR,

LAST Week I had an Occaſion of examining a curious Piece of *Roman* Antiquity diſcovered near us, and reſolv'd to ſend you the following Account of it, together with a Drawing I made of it.

In *Feb.* 1727-8. Plowing in the open Fields of *Denton*, about 2 ½ Miles from *Grantham*, they happen'd upon a *Roman Pavement* in *Moſaic* Work, as commonly call'd. I had Notice of it from my Neighbour, the Rev. Mr. *Saul*, Miniſter of *Harlaxton*, the next Pariſh, and went to ſee it. It lies partly in the glebe Land, partly in Madam *Welby*'s, who aſſiſted us with Workmen to clear it. It has been a very large Room about 30 Foot both ways, as we found by digging in divers Places; but being ſo near the Surface, not above a Foot, or a Foot and half deep, and having been plow'd

plow'd over Time out of Mind, the major Part of it is ruin'd and imperfect. Beſides many Fragments of it, we had only the Pleaſure of viewing one Piece entire, which was 30 Foot long and 6 broad; and this was extreamly pretty, the Colours lively, the Pattern or Figure finely deſign'd, as you will ſee by the Drawing, which is exactly taken. (*See the Fig.*) There are only 3 Colours, white, red and blue; but of the middlemoſt or moſt beautiful Part of it, which is but 9 Foot long and 3 broad, the white and red is Double in Quantity to the blue. In the outermoſt Part or Verge of the Work, there is no Variety of Colour, but 'tis entirely blue; and that made of much larger Squares than the reſt. On the *Eaſt* and *Weſt* Sides this was 6 Foot broad, on the *North* but 3. The red is formed out of *Roman Bricks*, ſeveral Fragments of which we found about the Work; the white Colour is made of the *common Lime-ſtone* of our Country; the blue, of the Stone that comes from *Benyngton* towards *Newark*, 5 Miles from this Place: And theſe Colours wear well together, and produce a good Effect. We found in digging, ſeveral Parts of the Foundations of the Walls that terminated this Room, and ſeemingly Foundations of other Rooms adjacent, which Foundations were made of the common white Stone of the Country ſet on Edge Side by Side, with here and there a Bit of *Roman* Brick. The Building was plac'd parallel with the Quarters of the Heavens. They found in digging, ſome human Bones, and I took many Bones of a Hand, which probably belong'd to ſome unfortunate Perſon kill'd in the Ruins, or when the Houſe was demoliſhed.

After this ſhort Deſcription of the Work, take this Account of the *Oeconomy* of the *Roman Times*, in

this Part of the Country, and I need detain you no longer. Three Miles South of *Grantham* is *Great Paunton*, a Village ſet in a ſweet Valley, where the Beginnings of the *Witham River* are collected from many Springs a little higher up. Hard by upon the Edge of the Hill runs the great *Roman* Road call'd *Hermen-ſtreet:* This Village I aſſerted in my Itinerary to be the *Cauſennis* in *Antoninus*'s Itinerary, which Commentators have found Difficulty in ſettling. The *Terms* or 2 *Towns* between which it is placed in *Iter* V. with the Diſtance of the Miles, ſufficiently eſtabliſh the Situation, and they are well known and acknowledged; thus *Durobrivas*, *Cauſennim* m. p. xxx. *Lindum* m. p. xxvi. for *Paunton* is 30 *Roman* Miles from *Durobrivæ*, or the Paſſage of the *Hermen-ſtreet* over the *Nen-river* above *Peterborough*, and 26 from *Lincoln*. At *Paunton*, ſince I came to live at *Grantham*, I have heard of much Antiquity being found, eſpecially *Moſaic Pavements*; and undoubtedly it was the *Station* upon the *Hermen-ſtreet*, between *Brigcaſterton* 12 *Roman* Miles off, and *Ancaſter* 7, which were likewiſe *Roman* Stations and wall'd about, but now their Names are loſt. All about *Paunton* the *Romans* inhabited very frequent, as at *Kirkſtoke* particularly, where great Quantities of Antiquities have been found; likewiſe at *Strawſton* I have ſeen many *Roman* Coins dug up, in Poſſeſſion of my Neighbour Captain *Hacket*, who owns the Town. He ſays likewiſe ſubterraneous Vaults have been found there, and near it is the Place where our Pavement was diſcovered. Between *Paunton* and *Grantham* a *Road* paſſes the River at *Salter*'s *Ford*, this Road is called *Salter*'s *Gate*, and much frequented. I believe it has continued ever ſince

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the *Roman* Times, being the Paſſage from *Holland* in *Lincolnſhire* by *Brigend Cauſey*, (a *Roman* Work) to all the Towns upon the *Foſſway* in *Nottinghamſhire* and *Leiceſterſhire*: For along this Way they carried Salt, made by the Sea Side, to thoſe mediterranean Parts, both in *Roman* and *Saxon* Times. Our Pavement ſtands within a Bow-ſhot of this Road, upon very high Ground, and ſeems to have been a *Summer-villa*, or Pleaſure-houſe, where on one Side they were entertained with the Sight of Travellers going on the Road; on the other they commanded a moſt noble Proſpect Northwards, of a prodigious Extent. When we are upon the Spot, you may without Difficulty ſee the Reaſon why they fixed it in that very Point, and 'tis the moſt delightful Place that can be imagined for a Summer-retreat. 'Tis placed juſt at the Head of a *Convallis*, or leſſer Valley falling down into the great Valley of *Denton* underneath, from which *Denton* has its Name, ſignifying the *Town in the Valley*. This *Convallis* is extreamly beautiful, and running Northward, muſt needs be very pleaſant and cool in the Summer-time. Our *Villa* had this further Advantage in its Situation, that it commanded a View between the Opening or Gap that in this Place appears, between *Barrowby* and *Wolſthorp Hills*; which leads the Eye into the boundleſs Proſpect of the Vale of *Belvoir*. You ſee from hence *Newark*, the *Trent*, *Kelham Park* and *Houſe*, *Southwell Minſter*, the *Foreſt* of *Sherwood*, &c. beſides the *neighbouring Caſtle of Belvoir*, where in theſe Times was a *Roman exploratory Camp*, likewiſe the *Roman* Camp of *Hunington*; inſomuch that we may well commend the Wiſdom and good Taſte of of the Builder, who contrived ſo well for *Security* and

Pleaſure. For *Air*, the Country hereabouts has always, and deſervedly, been reckoned the *Montpelier* of *England*; for *Water*, *Wood*, *Heath*, and *Proſpect*, it may be thought the *Freſcati*.

I am

Your moſt obedient Servant

and Brother,

William Stukeley.

III. *Some Reflections on Mr.* de Liſle's *Compariſon of the Magnitude of* Paris *with* London *and ſeveral other Cities, printed in the Memoirs of the* Royal Academy *of Sciences at* Paris *for the Year* 1725. *Communicated in a Letter to Dr.* Rutty, Secretary *to the* Royal Society, *by* Peter Davall, *of the* Middle Temple, *Eſq.*

MR. *de Liſle* in the Account he gives of his Method of making an exact Plan of *Paris*, and comparing it with *London*, and other Cities, firſt ſhews, by what Means he proceeded in determining, and laying down the true Situation of the ſeveral Places in *Paris*: After which he explains his Manner of drawing a true *Meridian Line* through that City; whereby he

he was enabled to divide it by *Meridians* and *Parallels*, as is practis'd in a general Map: And then he goes on in the following Words;

" *I traced the* Parallels *from* 15 *to* 15 *Seconds, and* " *the* Meridians *from* 20 *to* 20. *And, as under the* " *Parallel of* Paris, 15 *Degrees of* Latitude *are equi-* " *valent to* 20 *of* Longitude, *and the like is true of* " *Minutes and Seconds; by allowing* 5 *Seconds more* " *to the Intervals of the* Meridians, *than to those of* " *the* Parallels, *I form'd* perfect Squares."

He says, the chief Use he intended to make of these Squares, was to compare the Magnitude of *Paris* with that of *London*, and gives an Account of what Method he took to procure a just Plan of this City, which he reduced to the same Scale as that of *Paris*, and proceeds thus:

" *I traced upon it in like Manner*, Squares *from* " 15 *to* 15 *Seconds of a* great Circle, *and then I* " *was prepared to compare the Greatness of the two* " *Cities.*"

" *The Result of this Comparison is, that* Paris *con-* " *tains* 63 *of these Squares, which makes for its Su-* " *perficies* 3538647 *square* Toises: *And that* Lon- " don *contains only* 60 *of those Squares, or* 3370140 " *square* Toises."

And from hence he concludes, that *Paris* is *one twentieth* Part greater than *London*, tho' he says he has excluded several Gardens, contained within *Paris*, out of this Mensuration, which would have made it bear still a greater Proportion to *London*.

Upon reading this Account of Mr. *de Lisle*'s, it immediately occurred to me, that the Method which he has here taken of comparing the Magnitudes of *Paris* and

London, from whence he infers that the firſt of theſe Cities is *one twentieth* greater than the latter, is founded on a *falſe* Suppoſition, *viz.* That under the Parallel of *Paris* 20 Degrees of *Longitude* are equal to 15 of *Latitude*, and conſequently that by drawing Meridians from 20 to 20 Seconds, and Parallels from 15 to 15, the Figures formed by their Interſection will be *perfect Squares*: For the *Equator* and its *Parallels* are to each other as the *Sines* of their *reſpective Diſtances* from the *Pole*. Whence, as the *Radius*, or *Sine* of 90 Degrees, is to the *Sine* of the *Diſtance* of any *Parallel* from the *Pole*, or *Coſine* of its Latitude :: ſo is a *Degree* or any other Part of the *Equator*, or of any *great Circle*, to the *like Part* of the given *Parallel*. Therefore taking the mean Latitude of *Paris* at 48°. 51′, the Proportion of the Degrees of a *great Circle* to thoſe of the *Parallel* of *Paris* will by a Table of Sines be found to be as 1 to ,6580326. Whereas according to Mr. *de Liſle*, that Proportion is only as 20 to 15, or as 1 to .75. The Figures therefore which Mr. *de Liſle* calls Squares, are not ſuch, but *Rectangles*, whoſe longeſt Side containing 15 Seconds of a *great Circle*, bears the ſame Proportion to the ſhorteſt, containing 20 Seconds of the *Parallel* of *Paris*, as .75 does to .658, *&c.* or nearly as 8 to 7. And the Intervals, which he ought to have allowed to the Meridians, to make *perfect* Squares of theſe Figures, ought to have ben $\frac{15}{658}$ *&c.* Seconds, or nearly $22''\frac{4}{5}$ or $22''. 48'''$ of the *Parallel* of *Paris*.

Now Mr. *de Liſle* ſays, theſe Figures are *perfect Squares*, and has computed them as Squares, whoſe Side was $15''$ of a great Circle; for he ſays *Paris* contains 63 of theſe Squares, which makes 3538647 ſquare *Toiſes*,

Toiſes, which laſt Number being divided by 63, the *Quote* 56169 will be the Number of ſquare *Toiſes* contained in each Square, whoſe ſquare Root gives 237 *Toiſes* for the Side of each Square, which is juſt 15″ or $\frac{1}{240}$ of a Degree of a great Circle.

Mr. *de Liſle* hath therefore by this Account made the *ſuperficial Content* of each *Rectangle*, and conſequently of the whole City of *Paris* too great by near *one ſeventh*. To confirm which beyond Contradiction we have Mr. *de Liſle*'s own Teſtimony, who in the Plan he himſelf has drawn and publiſhed of *Paris*, and which he refers to in this very Account, has not made *Squares* of the above-mentioned Figures, but has given to their reſpective Sides the Proportion of 8 to 7, which is as near the true one as can well be expreſs'd by Lines, in a Plan of no larger a Scale than this.

Now in the Account we have been conſidering, Mr. *de Liſle* ſays himſelf, that in his meaſuring of *London* he drew *Squares*, whoſe Sides contained 15 Seconds of a *great Circle*, and of theſe he ſays, *London* contains *ſixty*.

Therefore to compare *Paris* with *London*, we ought for the foregoing Reaſons to make an Abatement out of the 63 Rectangles which *Paris* contains, nearly in the Proportion of 8 to 7; but becauſe that is a little greater than the true one, let us make ſuch Abatement only in the Proportion of 9 to 8, which is pretty conſiderably leſs than the juſt one. By which Abatement the Number of Squares, whoſe Side is 15 Seconds of a *great Circle* contained in *Paris*, will be reduced from 63 to 56. And conſequently, acccording to Mr. *de Liſle*'s own Way of meaſuring, the Magnitude of

London

London will be to that of *Paris* as 60 to 56, or as 15 to 14; or *London* will be *one fourteenth* greater than *Paris*. But to determine what Proportion theſe two Cities really bear to each other, requires a more exact Menſuration of *London* than any we yet have, which whoever would undertake, I think he cannot follow a better Method than that Mr. *de Liſle* has taken, and would adviſe him to conſult the Account upon which the foregoing Reflections are made, which he may find in the *Memoires* of the *Royal Academy of Sciences*, for the Year 1725. *pag.* 48.

IV. *An Account of an* Aneuryſm *of the* Aorta, (*diſſected in St.* Bartholomew's *Hoſpital*) *by* Pierce Dod, *M. D. Fellow of the College of Phyſicians, and Phyſician to that Hoſpital.*

AN Aneuryſm, without Doubt, is a Tumour ariſing from ſome Diſorder in an Artery; but what that Diſorder is, or whence it ariſes, is not ſo well agreed, the Accounts which are given of it, being widely different and uncertain.

The Name ſeems to imply, that it is a Dilatation of the Veſſel; but *Galen* deſcribes it to be a Tumour, which ariſes not from any Dilatation or Relaxation of an arterial Veſſel, and the Blood therein contain'd; but from an Extravaſation of the Blood from ſome Rupture of the Artery.

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Agreeable to this are the Opinions of all the reſt of the Antients, as likewiſe of the *Arabians*, who borrowed moſt that they have from them.

Fornelius, as 'tis ſaid, is the firſt who maintained, that the Artery was only dilated, and not burſt in an Aneuryſm, and that the Blood was contain'd within the Coats of it, as it is within thoſe of the Vein in a *Varix*; which is therefore called by ſome, ἡ φλὲψ ἀνευρυσμήνη.

Sennertus makes it to be a Dilatation, not of both the Coats, but of the outward one only, the inner or muſcular one being firſt burſten, or broken, and he is followed herein by moſt of thoſe who have ſucceeded him, excepting *Wiſeman* and ſome others, who tell us, that it is nothing but an Extravaſation of the Blood, burſting through the Coats of the Arteries into the Interſtices of the Muſcles, and there forming a Tumour ſuitable to the Cavity that it findeth, the Artery remaining undiſtended or undilated all the While; and that in all thoſe Aneuryſms, which have come to be examined, both the Coats of the Artery have conſtantly been found open.

This being the State of Opinions with relation to an Aneuryſm, we had lately an Opportunity of examining further into it, by Means of a Patient, who was taken into our Hoſpital.

She was about four and thirty Years of Age, and of a good Conſtitution, but there was a Tumour, bigger than one's Fiſt, which began from the upper Part of the *Sternum*, between the Origins of the *Muſculi Maſtoidæi*, and extended it ſelf to the *Pomum Adami*, almoſt up to her Chin, and poſſeſs'd all the Breadth between the two *Carotid Arteries*.

The Account that ſhe gave of the Occaſion of it was, that her Husband, being a paſſionate Man, took her by the Throat one Day as ſhe was crying out upon ſome Occaſion or other, and grip'd her ſo hard as almoſt to throttle her.

She was then with Child, and immediately perceiv'd ſomething of a Pain a little above her Heart, and a few Days afterwards there appeared a Tumour about the Bigneſs of the Top of her Finger, juſt above the *Sternum*, and ſo continued without Encreaſe or Pulſation, till ſhe was brought to Bed, when it began to be enlarged, upon her having a hard Labour; agreeable to what Practitioners have obſerv'd, that Accidents of this Nature often happen to Women in Labour.

This was about four Years ſince, and from that Time it had continued gradually encreaſing, until it was arrived to almoſt the higheſt Pitch of Extenſion; and ſhe had all along been troubl'd with a Palpitation, Pain and Straitneſs within the *Thorax*, great Interruptions in her Reſt, and frequent Sinkings, together with a conſtant beating along the Cheſt up to the Tumour; in which likewiſe there was a Pulſation correſpondent to the regular Pulſe, ſhaking the Tumour at every Stroke, and manifeſt to the Eye as well as the Touch. Notwithſtanding this ſhe was otherwiſe hearty, had her *Menſes* regularly, had a good Appetite, and was moſtly chearful and lively, and never more ſo than juſt before the fatal Period of the Tumour.

The *Apex* of the Tumour, which was towards the Middle, in the prominent Part of it was beginning to mortifie, through an over Diſtenſion, and the common outward Integuments were the firſt that ſeemed to ſuffer: But the Diſtenſion continuing, the Mortification encreas'd,

encreas'd, and was quickly communicated to the outer Coat of the Artery likewiſe, which therefore ſlough'd off as well as the other Integuments, and being at Length wore away, juſt at the Extremity made a ſudden Aperture, about twice the Bigneſs of a Gooſe's Quill. The Blood inſtantly guſh'd forth, as from a Stream or Torrent, and the poor Patient died in leſs than a Minute.

Upon opening the Body, we began from the Heart, in which there was little remarkable, except that the left Ventricle was ſomewhat larger, as were likewiſe the *Columnæ carneæ*, than they naturally ſhould be. There was little obſervable likewiſe in the *Aorta* itſelf, till we came to the Curvature; upon the upper Side of which was the *Baſis* of the Tumour, forming a cylindrical Stem of four Inches long while in the Cavity of the *Thorax*; but extending it ſelf into a circular Form of a larger Dimenſion, when it became external.

Upon opening the under Part of the *Aorta* oppoſite to this *Baſis*, and carrying the Inciſion throughout its whole Extent in the *Thorax*, the Trunk retain'd its uſual Form and Dimenſions, and was not at all dilated; but in the upper Part above deſcrib'd, juſt on this Side the Orifice of the right *ſubclavian Artery* (which was nearer than uſual to the Orifice of the left *Carotid*) there was a preternatural circular Aperture of half an Inch Diameter. Upon dividing this Aperture, and carrying on the Inciſion to the *Apex* of the Tumour, its whole internal Subſtance appear'd. The Edges of the Aperture at the Baſis of the Tumor were hard, and almoſt cartilaginous, and ſeemingly the Remains of thick and fleſhy Fibres; which upon a nicer Inſpection they appear'd to be in Fact, *viz.* the broken Fibres of the in-

ner, or what is commonly call'd, the muſcular Coat of the Artery: which terminating here, the Tumour immediately encreas'd to 2 Inches in Diameter, and continued of that Dimenſion, till it came out at the Neck, between the Clavicles; but then extended it ſelf circularly to a Diameter of above 3 Inches, the Covering of which was nothing elſe but the outer Coat of the ſame Artery all along dilated from the Baſe, even to the Extremity of the Tumour.

The Cavity was for the moſt Part fill'd with a Sort of *Polypus*, or *Sarcoma*; in which nevertheleſs there were three Sinuſes, or Paſſages, that were kept open by the conſtant Influx of the Blood, and communicated near the *Apex* with one another; (that in the Middle being the largeſt,) and terminating in one towards the Extremity of the Tumour, not far from where it broke.

Such was the State of the Aneuryſm in this Subject, what it may be in others, future Enquiries muſt determine.

V. *Some Obſervations on* Aneuryſms *in general, and in particular, on the fore-going.* *By* F. Nicholls, *M. B. Præl. of Anatomy,* Oxon. *& F. R. S.*

AN Aneuryſm is by all Authors defin'd to be a ſoft circumſcrib'd Tumor, in which there is a ſenſible Pulſation, cotemporary with the Pulſation of the Artery, to which it adheres. As it is certain, that any Tumor of what Kind ſoever, lying on, or adhering to any

any conſiderable Artery, muſt neceſſarily be moved by every Pulſation of ſuch Artery, ſo this Pulſation (unleſs underſtood in ſuch Manner as I ſhall hereafter explain) can no ways be admitted as the true Diagnoſtick, whereby to ſpecify the Difference between this Kind of Tumor and any other.

An Aneuryſm is found moſt commonly to ſucceed Falls, Vomitings, Labour-ſtrains, and ſuch other Motions or Indiſpoſitions of the Body as, by compreſſing the great Branches of an Artery, any ways ſtop the progreſſive Motion of the Blood.

It is obvious that, as the Section of the Artery above the Compreſſure muſt in its natural State be ſometimes very incapable of containing at once the whole Quantity of Blood, which ought only to have paſs'd thro' it ſucceſſively; and as the Force of the Heart may frequently exceed the Reſiſtance it may meet with from the Coats of the Artery; ſo the Conſequence of ſuch a Stop to the progreſſive Motion of the Blood, may occaſion either a *Rupture* of the Artery, or a *Diſtenſion* of the Artery without a Rupture, or a *Rupture* of the *internal* Coats of the Artery, and a *Diſtenſion* of its *external* Coat.

A Rupture of the large Branches of the *Aorta* neceſſarily allow ſo plentiful Effuſions of the Blood, as to occaſion immediate Death; while the Capillaries may be burſt without any other Injury, but a ſlight *Echymoſis*, and the Tumor form'd by the Effuſion from them will be diffuſed and ſuperficial.

A Rupture of the mean Branches (ſuch I intend, as deſcend between the *Tibia* and *Fibula*, the *Radius* and *Ulna*, &c.) will be attended with a conſiderable Effuſion of Blood; but as the Blood will find a Paſſage

between the Interſtices of the Muſcles, it will never form a circumſcrib'd Tumor. However, the Effuſion being continued *per ſaltum* thro' the ruptur'd Artery, will give a faint Pulſation, and conſequently ſome Reſemblance of the Aneuryſm; for which Reaſon it is by ſome Chirurgeons term'd a Baſtard-Aneuryſm.

Whether or no an Aneuryſm be a Tumor form'd by the *Dilatation* of the Artery, or by a *Rupture* of the *internal* Coats of the Artery, and a *Diſtenſion* of the *external*, has for ſome Time been a Matter of great Diſpute; each Party proteſting (perhaps too unjuſtly) againſt the Poſſibility of the others Opinion.

As to the Poſſibility of an Artery's being dilated, it ſtands ſupported by Reaſon and Autopſy. We find the Uterine Arteries conſtantly encreas'd in Thickneſs and Diameter, in Proportion as the *Uterus* is diſtended; and many Caſes of Palpitations of the Heart have been attended with great Dilatations of the *Aorta*; Inſtances of which I have ſeen both in human and brute Subjects.

Such a Dilatation will neceſſarily follow a conſtant, or frequent Preſſure on any Part of the *Aorta*, provided ſuch Preſſure does not entirely ſtop the progreſſive Motion of the Blood thro' the *Aorta*.

But on the other Hand, ſuch a Dilatation will always retain ſomewhat of the Form of the Artery. The Reſiſtance will not be every Way equal, as in the extravaſate Tumors; becauſe the quaquaverſal Preſſure of the Blood will be controll'd by the Preſſure on the Artery, and the Reſiſtance from the Coats of the Arteries, ſo as neceſſarily to form a Cylindroeid. And the Conſequence of ſuch a Dilatation cannot (if con-

conſider'd abſtractedly from its Preſſures) be worſe (if ſo bad) than from a varicous Vein.

Again, they who conceive an Aneuryſm to be a *Rupture* of *both Coats* of the Artery, oppoſe their Opinion, who imagine the *internal* Coat to be *ruptured*, and the *external* to be *diſtended*, by comparing the two Coats in Queſtion, and urging, that, as the internal Coat is ſo much thicker than the external, it ſeems impoſſible the laſt ſhou'd be ſufficient to reſiſt a Force capable of deſtroying the firſt. Were theſe two Coats *ſimilar* as to their Structure, we might then compute their Strength by their Thickneſs, and this Argument wou'd be of much greater Force than at preſent it can be; becauſe the internal Coat being compoſed of *annular Faſciculi*, whoſe Sides have but a very weak Coheſion, their Power of reſiſting will not be meaſurable by the Strength of thoſe *Annuli*; but by the Force with which they adhere *laterally*. And on the other Hand, the external Coat being compoſed of Fibres *equally interwoven*, and of a quite different Compoſition, it may either exert a greater Reſiſtance, or be capable of much greater Dilatations than the internal.

But that *Autopſy* may evince the Truth of this Difference in the Strength of theſe Coats, it will be found by any one who pleaſes to try the Experiment, that by blowing into the Pulmonary Artery, the internal Coat will ſoon burſt, and the external form it ſelf into aneuriſmous Tumors, (*which Experiment was accordingly try'd before the Society, to their Satisfaction.*)

Upon conſidering all which, and having, by *Order of the Society*, both privately and publickly examin'd the Aneuryſm before us, which I find to be round like other extravaſate Tumors, unleſs when controuled by

any

any notable Preſſure, and that the *Sacculus* does not *divide* into *Coats* as the Artery from whence it ariſes *does*, I am induced to think that this Aneuryſm is a Tumor form'd by the Blood's being *forced thro'* the *ligamentous*, or what is called the Muſcular-coat, and *diſtending* the *membranous* or outer one. And becauſe the Impetus of the Blood will, as it were, perpetually preſs thro' the Aperture into the Tumor, and be again (at leaſt in Part) return'd by the Elaſticity of the external Coat; therefore ſuch a Tumor will rather have a *pulſatile* Dilatation, than a *Pulſation*, for its true Diagnoſtick.

VI. *A Letter to Dr.* Halley, *Aſtron. Reg. & F. R. S. relating to a ſurpriſing Shoal of* Pumice-Stones *found floating on the Sea, by Mr.* John Dove.

SIR,

HAving examined my Journal, I ſend you herein a particular Account of what I can remember, concerning the *Pumice-ſtones* we fell in with, in our Voyage to *India* in the *Lyell*, *Charles Small* Commander.

On *Monday the* 22^{d} of *March*, 172$\frac{4}{5}$, at Noon, being in the Latitude 35°, 36′ South, and Longitude 4°, 9′ Weſt, with Variation 3°, 16′, W. we diſcovered ſeveral *Pumice-ſtones* on the Sea; but not expecting any ſuch thing

thing at that Diſtance from the Land, (the Iſlands *Triſtan d' Acunha* being the neareſt, which I judged to bear from us W. 9°, 10′, S. Diſtance 186 Leagues) we were in Diſpute what it might be; when about 1 P. M. we took up a Piece in a Bucket, (the Ship going then but 3 Knots) which confirmed my Opinion of its being *Pumice-ſtones*, ſuch as I gave you. Towards Night it was ſpread all round us as far as we could ſee: The Wind being variable from N. by E. to E. we ſtood to the Eaſtward: Towards Morning, the Wind veering to the Northward, we ſteered E.S.E. The *Pumice-ſtones* were very thick, in Drifts, lying N. N.E. and S. S. W. and extended out of our Sight at the Maſt's Head, encreaſing as we ran to the Eaſtward. To the 23d at Noon I made our Courſe S. 38°, 30′ E. Diſtance 76 Miles: Latitude by Obſervation 36°, 35′ S. Longitude 3°, 24′ Weſt, from the Meridian of *London*.

Wedneſday the 24th, clear Weather and freſh Gales, variable from N.E. by N. to N. by W. with a long Swell from the Eaſtward. We continued our Courſe E.S.E. 140 Miles, the *Pumice-ſtones* being thicker; ſo that from Yeſterday Noon till four this Morning, ſome of the Drifts were about a Cable's Length broad, and ſo thick, we could ſcarce ſee the Water between them; and there was much the ſame Breadth between the Drifts, with ſeveral *Pumice-ſtones* interſperſed. Towards Noon, I found the *Pumice* ſomewhat thinner: Latitude 37°, 35′ S. and Longitude 1°, 4′ W.

Thurſday the 25th, from Noon till two this Morning, had a freſh Gale at N. and N. by E. afterwards little Wind from W. to N. W. with a N. E. Sea: we ſteered E. by S. 101 Miles. In the Evening the Drifts were near as large as above, but towards Morning decreaſed much;

much; ſo that about Noon we were clear of theſe *Pumice-ſtones*, ſeveral of which were as big as a Man's Head. We have ſailed 317 Miles ſince we firſt diſcovered them. They lay juſt in the Track for Ships outward bound, and we have no Account of them before; but all the Ships that went out the ſame Year, and ſince, (who go ſo far to the Southward) have fallen in with them. In the Morning we tried the Current, but found none: and no Ground at 130 Fathoms. At Noon Latitude 37°, 54′ S. Longitude 0°, 38′ E. The following Evening Variation 6°, 12′ W. At Noon, I judge *Triſtan d' Acunha* then bore from us W. 3°, 39′ N. Diſtance 256 Leagues, ſuppoſing it to lie in Latitude 37°, 5′ S. and Longitude 15°, 38′ W.

As to the Original of the *Pumice-ſtones*, I ſubmit that to your better Judgment.

If I can oblige you in any thing elſe, you may command,

SIR,

Your moſt humble Servant,

Eaſt-Lane, Rotherhith,
Febr. 27th. 1727-8.

John Dove.

VII. *Some*

VII. *An Account of ſome Obſervations made by a young Gentleman, who was born blind, or loſt his Sight ſo early, that he had no Remembrance of ever having ſeen, and was couch'd between 13 and 14 Years of Age.* By *Mr.* Will. Cheſſelden, *F. R. S. Surgeon to Her* Majeſty, *and to St.* Thomas's *Hoſpital.*

THO' we ſay of the Gentleman that he was blind, as we do of all People who have Ripe Cataracts, yet they are never ſo blind from that Cauſe, but that they can diſcern Day from Night; and for the moſt Part in a ſtrong Light, diſtinguiſh Black, White, and Scarlet; but they cannot perceive the Shape of any thing; for the Light by which theſe Perceptions are made, being let in obliquely thro' the aqueous Humour, or the anterior Surface of the Chryſtalline (by which the Rays cannot be brought into a *Focus* upon the *Retina*) they can diſcern in no other Manner, than a ſound Eye can thro' a Glaſs of broken Jelly, where a great Variety of Surfaces ſo differently refract the Light, that the ſeveral diſtinct Pencils of Rays cannot be collected by the Eye into their proper *Foci*; wherefore the Shape of an Object in ſuch a Caſe, cannot be at all diſcern'd, tho' the Colour may: And thus it was with this young Gentleman, who though he knew theſe Colours aſunder in a good Light; yet when he ſaw them

after he was couch'd, the faint Ideas he had of them before, were not ſufficient for him to know them by afterwards; and therefore he did not think them the ſame, which he had before known by thoſe Names. Now Scarlet he thought the moſt beautiful of all Colours, and of others the moſt gay were the moſt pleaſing, whereas the firſt Time he ſaw Black, it gave him great Uneaſineſs, yet after a little Time he was reconcil'd to it; but ſome Months after, ſeeing by Accident a Negroe Woman, he was ſtruck with great Horror at the Sight.

When he firſt ſaw, he was ſo far from making any Judgment about Diſtances, that he thought all Objects whatever touch'd his Eyes, (as he expreſs'd it) as what he felt, did his Skin; and thought no Objects ſo agreeable as thoſe which were ſmooth and regular, tho' he could form no Judgment of their Shape, or gueſs what it was in any Object that was pleaſing to him: He knew not the Shape of any Thing, nor any one Thing from another, however different in Shape, or Magnitude; but upon being told what Things were, whoſe Form he before knew from feeling, he would carefully obſerve, that he might know them again; but having too many Objects to learn at once, he forgot many of them; and (as he ſaid) at firſt he learn'd to know, and again forgot a thouſand Things in a Day. One Particular only (tho' it may appear trifling) I will relate; Having often forgot which was the Cat, and which the Dog, he was aſham'd to aſk; but catching the Cat (which he knew by feeling) he was obſerv'd to look at her ſtedfaſtly, and then ſetting her down, ſaid, So Puſs! I ſhall know you another Time. He was very much ſurpriz'd, that thoſe Things which he had lik'd beſt,

beſt, did not appear moſt agreeable to his Eyes, expecting thoſe Perſons would appear moſt beautiful that he lov'd moſt, and ſuch Things to be moſt agreeable to his Sight that were ſo to his Taſte. We thought he ſoon knew what Pictures repreſented, which were ſhew'd to him, but we found afterwards we were miſtaken; for about two Months after he was couch'd, he diſcovered at once, they repreſented ſolid Bodies; when to that Time he conſider'd them only as Party-colour'd Planes, or Surfaces diverſified with Variety of Paint; but even then he was no leſs ſurpriz'd, expecting the Pictures would feel like the Things they repreſented, and was amaz'd when he found thoſe Parts, which by their Light and Shadow appear'd now round and uneven, felt only flat like the reſt; and aſk'd which was the lying Senſe, Feeling, or Seeing?

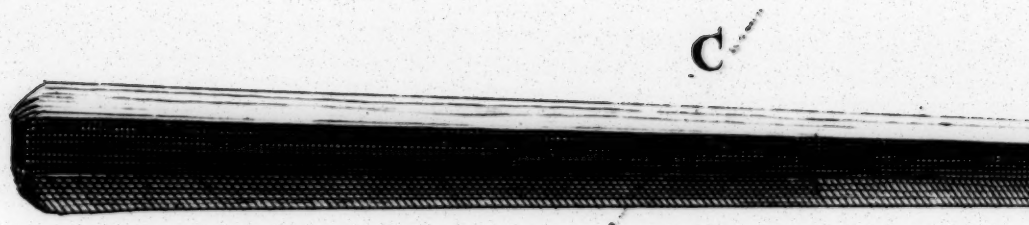
Being ſhewn his Father's Picture in a Locket at his Mother's Watch, and told what it was, he acknowledged a Likeneſs, but was vaſtly ſurpriz'd; aſking, how it could be, that a large Face could be expreſs'd in ſo little Room, ſaying, It ſhould have ſeem'd as impoſſible to him, as to put a Buſhel of any thing into a Pint.

At firſt, he could bear but very little Sight, and the Things he ſaw, he thought extreamly large; but upon ſeeing Things larger, thoſe firſt ſeen he conceiv'd leſs, never being able to imagine any Lines beyond the Bounds he ſaw; the Room he was in he ſaid, he knew to be but Part of the Houſe, yet he could not conceive that the whole Houſe could look bigger. Before he was couch'd, he expected little Advantage from Seeing, worth undergoing an Operation for, except reading and writing; for he ſaid, He thought he could have no more Plea-

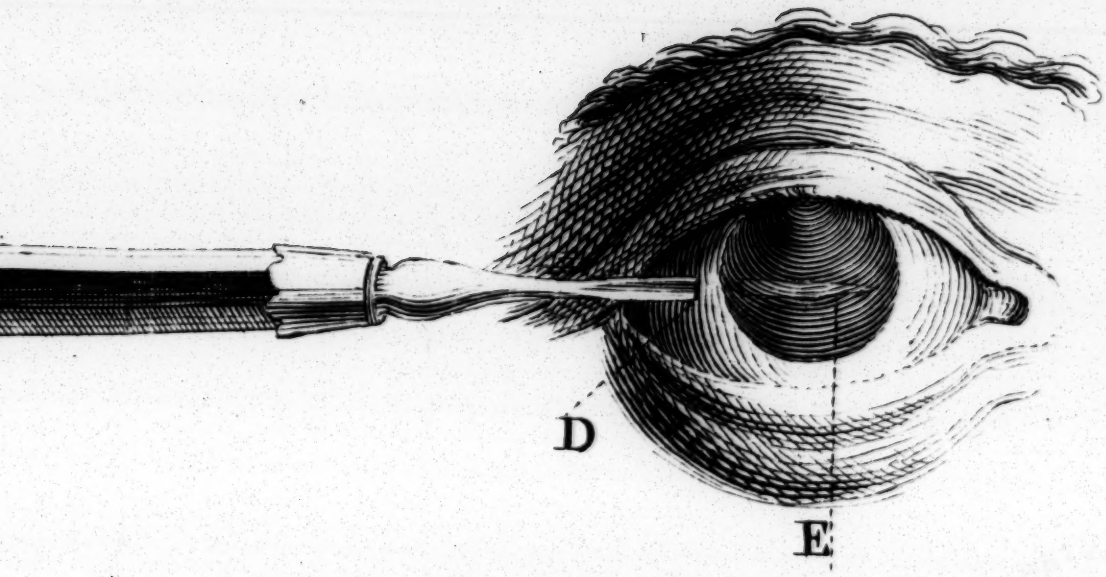
ſure in walking abroad than he had in the Garden, which he could do ſafely and readily. And even Blindneſs he obſerv'd, had this Advantage, that he could go any where in the Dark much better than thoſe who can ſee; and after he had ſeen, he did not ſoon loſe this Quality, nor deſire a Light to go about the Houſe in the Night. He ſaid, every new Object was a new Delight, and the Pleaſure was ſo great, that he wanted Ways to expreſs it; but his Gratitude to his Operator he could not conceal, never ſeeing him for ſome Time without Tears of Joy in his Eyes, and other Marks of Affection: And if he did not happen to come at any Time when he was expected, he would be ſo griev'd, that he could not forbear crying at his Diſappointment. A Year after firſt Seeing, being carried upon *Epſom Downs*, and obſerving a large Proſpect, he was exceedingly delighted with it, and call'd it a new Kind of Seeing. And now being lately couch'd of his other Eye, he ſays, that Objects at firſt appear'd large to this Eye, but not ſo large as they did at firſt to the other; and looking upon the ſame Object with both Eyes, he thought it look'd about twice as large as with the firſt couch'd Eye only, but not Double, that we can any Ways diſcover.

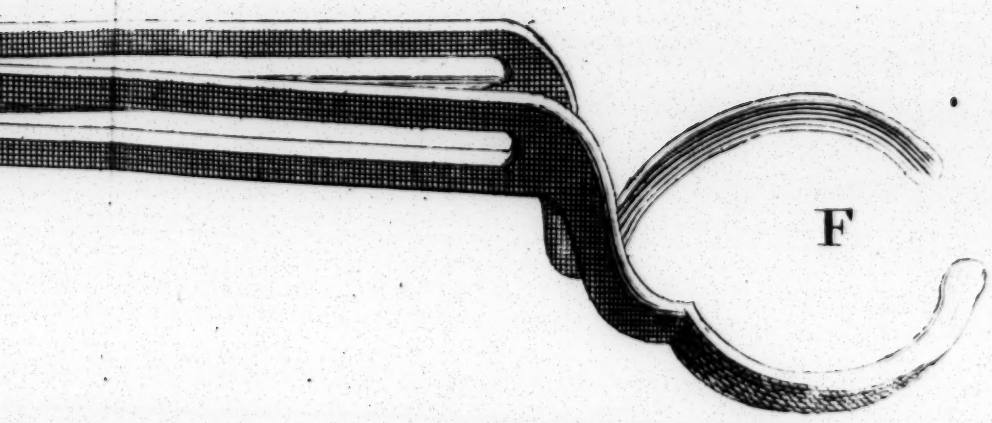
VIII. *An*

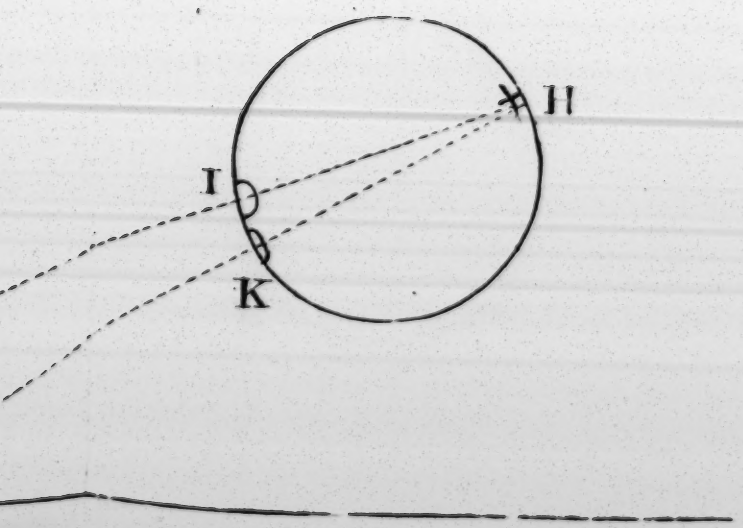
Plate II :



B


D
E


F


H
I
K

VIII. *An Explication of the Inſtruments uſed, in a new Operation on the Eyes, by the* Same.

A B REpreſent the Figures of two Eyes, on which a new Operation was perform'd, by making an Inciſion thro' the *Iris*, which had contracted itſelf in both Caſes ſo cloſe, as to leave no Pupil open for the Admiſſion of Light. (*See Plate the* 2d.)

The Perforation in the Eye A was made a little above the Pupil, the cloſing of which enſued upon the putting down a Cataract, which not knowing how low it might be lodged, I made the Inciſion a little higher than the Middle, leſt any Part of it ſhould lie in the Way.

The Eye B was one I couch'd not long before, where the Patient had been blind but a few Years. At firſt he thought every Object further from him than it was; but he ſoon learn'd to judge the true Diſtance, the Cauſe of which I ſhall endeavour to explain by the lowermoſt Figure, in which let the Circle H I K repreſent the Eye, H the Place where an Image through the natural Pupil I was repreſented from the Place M; now the artificial Pupil being at the Place K, the Object at L is now painted at the Place H, where the Object M was alſo to be perceiv'd; therefore it was, I ſuppoſe, that the Patient miſtook the Place L for the Place M.

C is a Sort of Needle with an Edge on one Side, which being paſs'd thro' the *Tunica Sclerotis*, is then brought forwards thro' the *Iris* a little farther than E. This done, I turn the Edge of the Needle, and cut thro' the *Iris* as I draw it out: The Handle of this Needle is half black, and half white, which though it is not of much Uſe in this Operation, is very much ſo in couching Needles, we being thereby able to judge of their Poſition, when we do not ſee them.

F F is an Inſtrument to keep open the Eye-lids. G is a Bit of Iron, which as it is moved backward, or forward, the Inſtrument opens and cloſes.

IX. *An Account of ſeveral Stones found in the Kidneys of a Perſon opened by Mr.* John Dobyns, *Surgeon and Lithotomiſt to St.* Bartholomew's *Hoſpital and F. R. S.*

MR. *Laurence*, a Gentleman of about forty Years of Age, had for near twenty Years a Complaint in his Kidneys; making bloody Urine upon any extraordinary Motion, but free from the great Pain, and all other Symptoms uſually attending nephritick Caſes. However, upon opening the Kidneys after his Death, there was in each a Stone of an extraordinary Size and Figure, beſides one hundred ſmaller, which are brought for the View of the Society,

Plate 3.

Fig. 2.

Fig. 1.

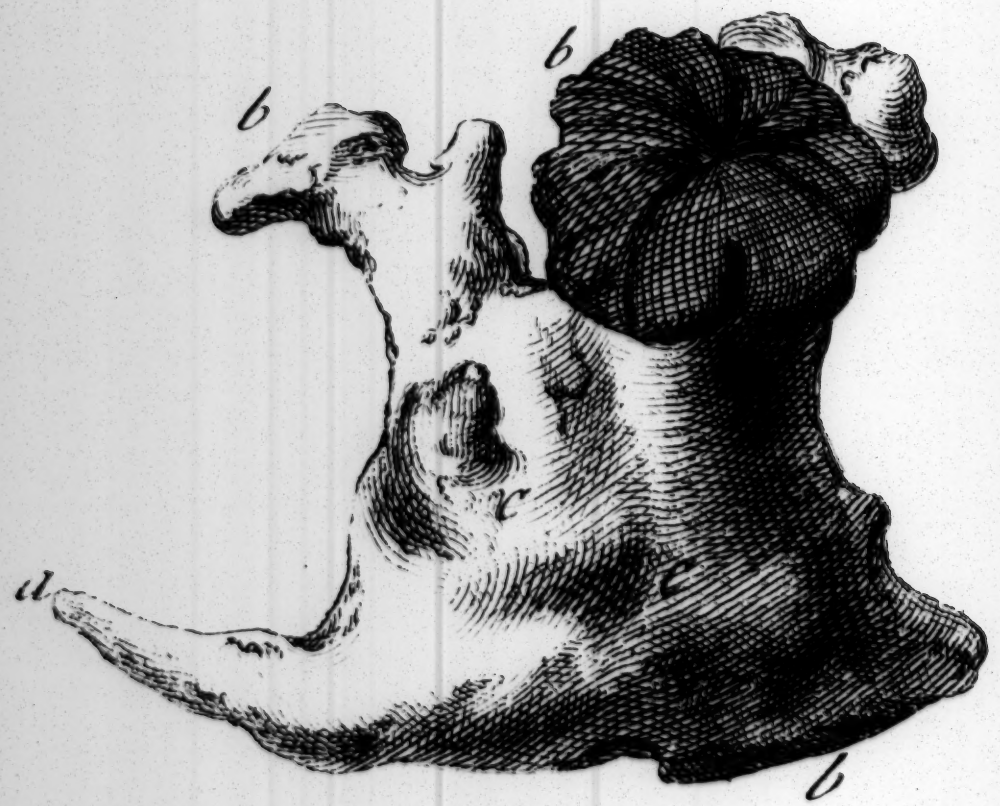
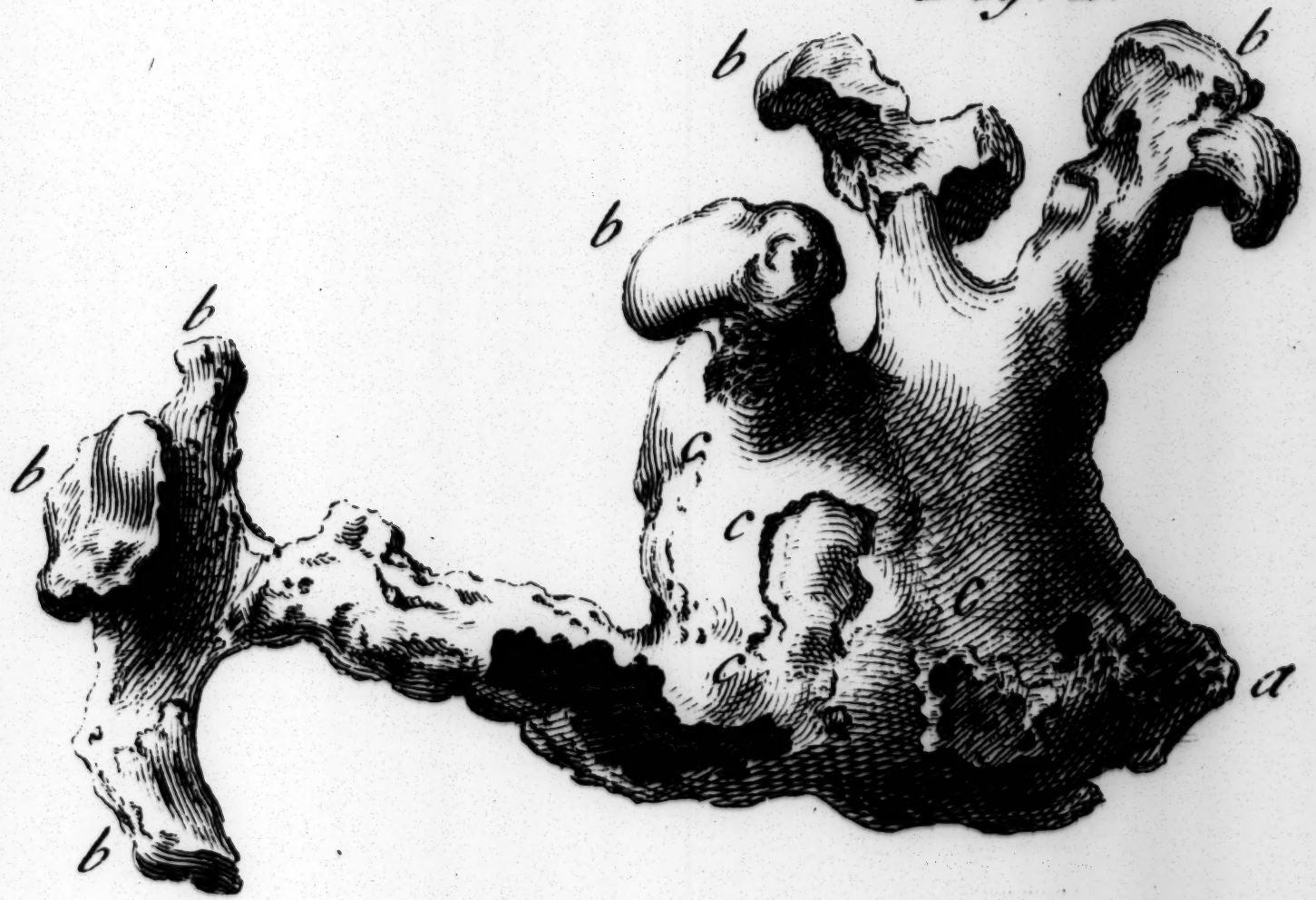


Fig. 2.



Society, with a Deſcription and Draught of the two largeſt ſubjoin'd.

Fig. I. Shews the Stone denudated, as taken from the *Pelvis* of the right Kidney. (*See Plate the third, Figure the firſt*)

a. That Part which had branch'd into the *Ureter*, and totally obſtructed its Channel. *b. b. b.* The eminent Parts of it. *c. c. c.* That Part which fill'd the Capacity of the *Pelvis*.

Fig. II. Shews the Stone taken from the left Kidney. (*See Plate the third, Figure the ſecond.*)

a. That Part which had protruded it ſelf into the upper Part of the *Ureter*, but did not totally plug it up, by which Means the Urine had a Paſſage. *b. b. b. b.* The eminent Parts which branch'd into the *Fiſtulæ membranaceæ*. *c. c. c. c.* The Body of the Stone which lay in the *Pelvis* of this Kidney.

X. *Extract of ſeveral Letters* &c. *from different Parts of* Europe, *relating to the* Aurora Borealis *ſeen* Oct. 19. N. S. 1726.

THough the Accounts referr'd to have come to Hand very late, long ſince the Appearance of the Meteor, yet as it is manifeſtly the ſame with that remarkable one which was ſeen the ſame Night here,

(*viz.* Oct. 8. O. S.) the Deſcription of which is publiſh'd in the *Tranſaction* N°. 395, it will not be amiſs to give a ſhort Extract of them, that the Extent of the *Phenomenon* may be more fully known, and its Height thereby better eſtimated.

Monſ. *Gaudin*, in a Letter from the Obſervatory at *Paris*, dated *October* 20. N. S. 1726. writes, that he ſaw it firſt at half an Hour paſt ſeven in the Evening, forming at that Time a luminous Arch (with another ſomewhat darker under it) which extended it ſelf almoſt from *Sun-ſet* to *Moon-riſe*, and was raiſed above the *Horizon* about twenty five Degrees; from whence ſhot out from Time to Time luminous Streams about ten Degrees above it. At half an Hour paſt eight, the Number of theſe Streams vaſtly encreas'd, covering all the Heaven, excepting the Height of twenty Degrees oppoſite to it: But towards the *Zenith* there remain'd a circular Space which was never cover'd by them, tho' there wanted not a conſtant Succeſſion. Theſe Appearances continu'd very ſtrong till half an Hour after ten; when they began to decline, and diſappear'd totally about two in the Morning.

Monſ. *Maraldi*, in a Letter dated at *Thiers*, *Oct.* 20, 1726. N. S. two Leagues to the South of *Paris*, ſays, it began there about half an Hour paſt ſix with a conſtant uniform Light in the *North*; ſoon after which appear'd three or four luminous Arches one over another, from whence iſſued a great Number of Rays, which ſhot up a conſiderable Height above the *Horizon*. At eight o' Clock theſe Rays darted quite up to the *Zenith*; half an Hour after which they very much encreas'd, ſpreading with ſtrong Undulations all over the

the Sky, and all terminating in the *Zenith* form'd a Sort of *Cupola* there. The Concluſion he has not obſerv'd.

Sign. *Franceſco Quaranbotti* writes from *Treggiaia*, *Oct.* 20. 1726. N. S. that he firſt obſerv'd it a little before eight in the Evening, when it extended it ſelf along the North Horizon about eighty Degrees, and reach'd above it about eight. After ſome Time, the luminous Emiſſions began to riſe perpendicularly, and continued from time to time ſo to do, from nine till eleven. About ten it enlarged it ſelf fifteen Degrees farther *Eaſt*, and ſtretch'd under the laſt Star in *Urſa major*. At eleven it vaniſhed.

An anonymous Account in *Latin* from *Florence* informs us, that it was firſt ſeen there at half an Hour paſt ſix in the Evening, with a clear expanded Light, occupying all the Space betwixt the *North-Eaſt* and *North-Weſt*. At ſeven it divided it ſelf into ſeveral ſpherical Triangles near the *Horizon*, which half an Hour afterwards united into one large one, whoſe Baſe was near the *Horizon*, and extended twenty Degrees to the *Weſt* from the *North-Pole*, and whoſe *Vertex* reach'd up to *Urſa minor*. This continu'd about half an Hour, and then diſappear'd; but at ten o' Clock it return'd much more conſpicuouſly, forming about the *Pole*, a large Column which was rais'd thirty Degrees above the *Horizon*. From this Time it iſſued out lucid Undulations till Midnight, when it entirely diſpers'd. He afterwards takes Notice that the ſame was ſeen at *Milan* and *Bologna*; the Accounts from whence agree, that none of the Streams reach'd beyond the *Zenith*.

Sign. *Manfredi* writes from *Bologna, Jan.* 3. 1726-7. that he did not obſerve this *Phenomenon* himſelf, but was inform'd that it was ſeen every where in the *Campagna di Roma*, as far as *Peſaro* and *Fano*.

To theſe Accounts, which were communicated to the Society by their worthy Aſſociate Sir *Tho. Dereham*, and moſt of them tranſlated from the *Italian* by the ingenious Dr. *Sheuzer*, it will not be improper to ſubjoin, that Dr. *Ericus Burman* in the *Act. Literar. Suec. Trimeſt. prim.* 1727. takes Notice, that altho' this *Meteor* was ſeen in *Germany*, *Poland*, *Swiſſerland*, *France*, and *England*, yet at *Upſal* they cou'd obſerve nothing but the whole Sky beſet thick with Clouds, of a Colour like that of the Moon in a total Eclipſe, and variouſly agitated as by a Wind, but this chiefly towards the *South*; which continu'd till nine o' Clock at Night, a little after which it grew quite cloudy.

FINIS.

LONDON: Printed for WILLIAM INNYS, at the *Weſt End* of St. *Paul*'s.
M.DCC.XXVIII.

Philo: Trans: N.o 403

Solaris Eclipsis o

Macul

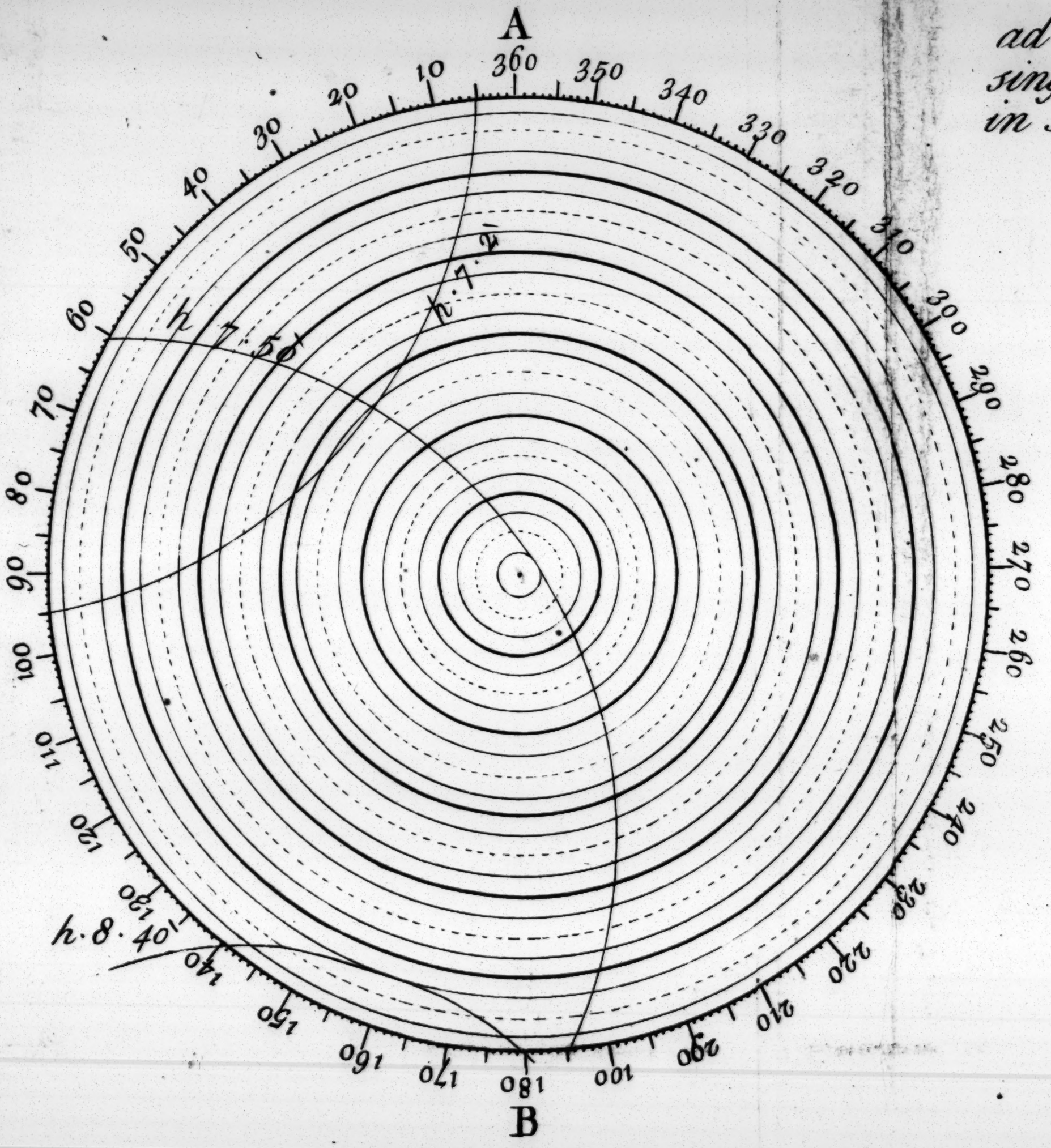
s

ad ex

singu

in se

AB *planum Circuli Verticalis per centrum Solis ducti*

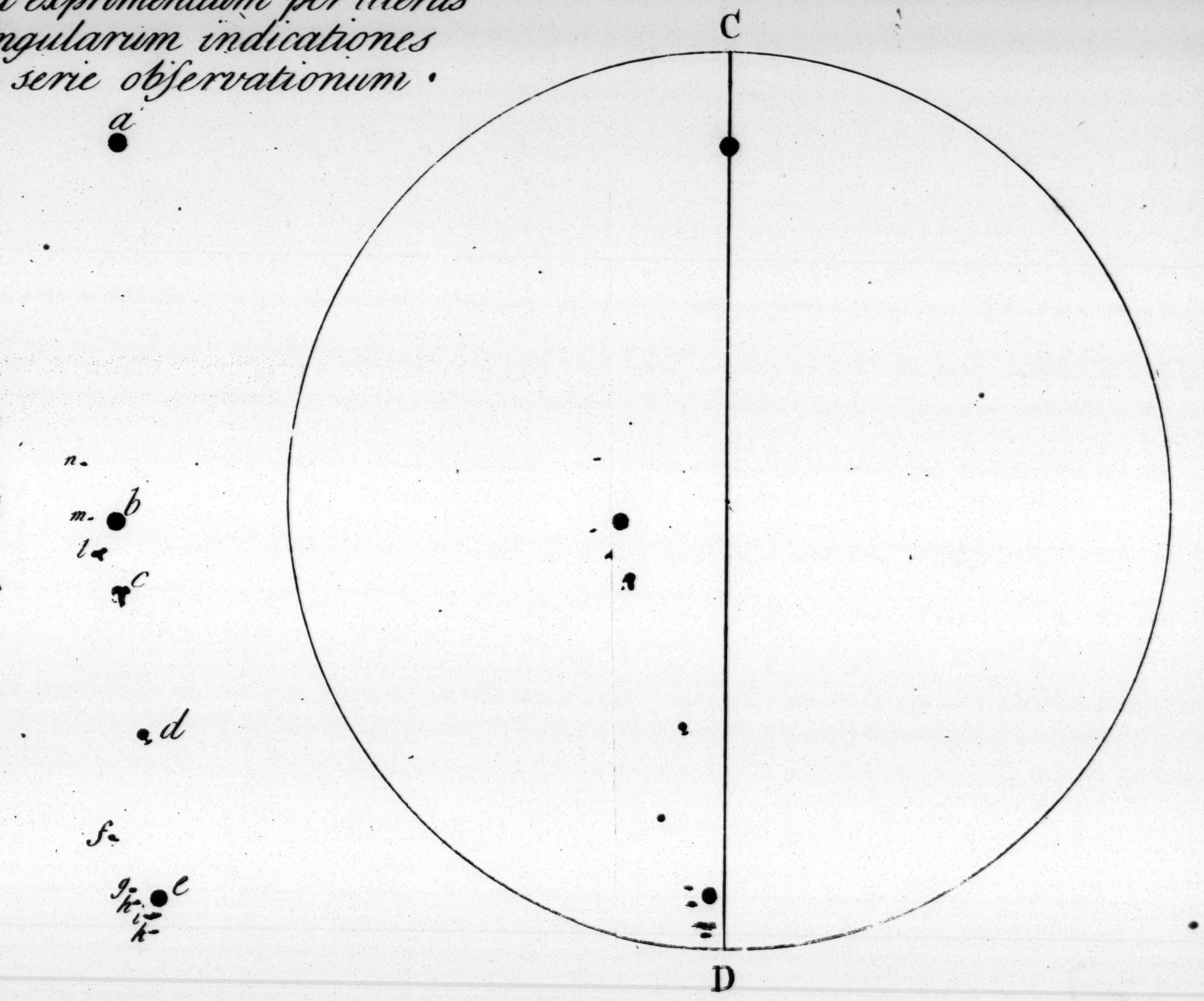


Discus Solis in digitos **XII** *et quadrantes digitorum divis*
in quo gradus peripheriæ 360 ab apice **A**
sinistrorsum procedendo notantur, ut per singulas phas
interfectio communis cum disco Lunæ indicari possit

s observata Romæ die 15 Septembr: 1727 .. V. S.

culæ in figura Solaris Disci CD
suis locis collocatæ
hic repetuntur
d exprimendum per literas
ngularum indicationes
serie observationum.

CD planum circuli Verticalis
per centrum Solis ducti



ivisus

haſes

Maculæ in disco Solis
per Telescopium in chartam excepto
ita erant constitutæ

Philō Transˉ N.º 403.

Plate 2.

Fig. 4.

Fig. 6.

Fig. 5.

Fig. 7.

Fig. 8.

Fig. 9.

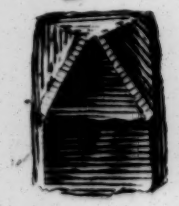
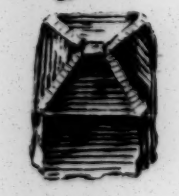


Fig. 10.

Fig. 11.



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Numb. 403.

PHILOSOPHICAL TRANSACTIONS

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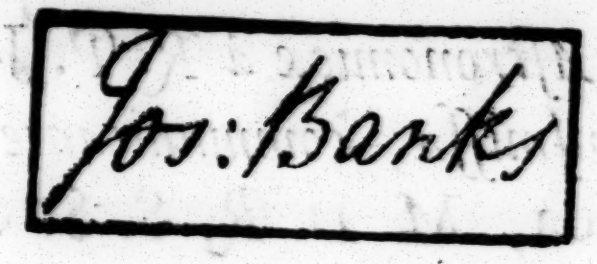

I. *An*

E R R A T U M.

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Plate 1.

Fig. 1.

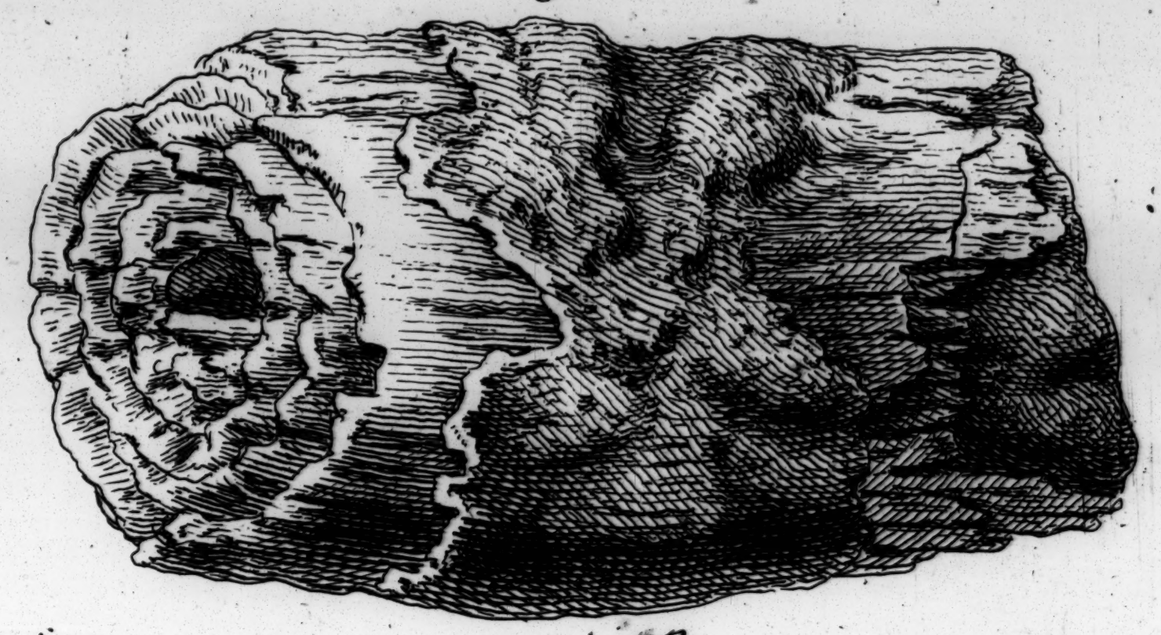


Fig. 2.

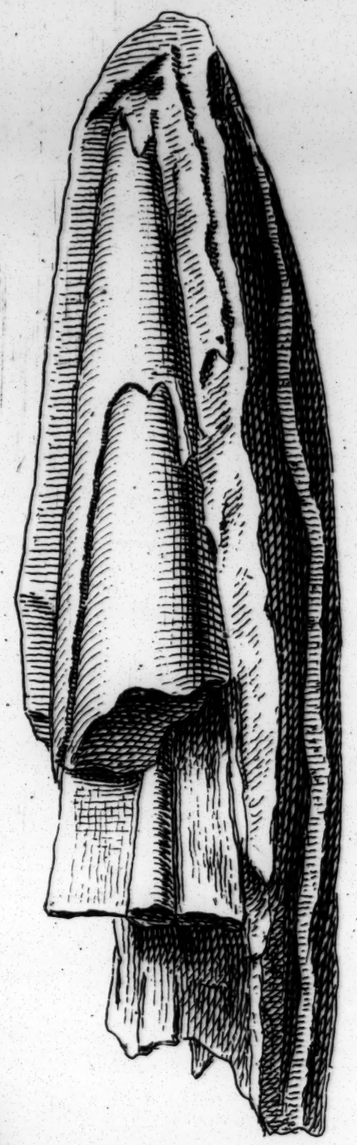
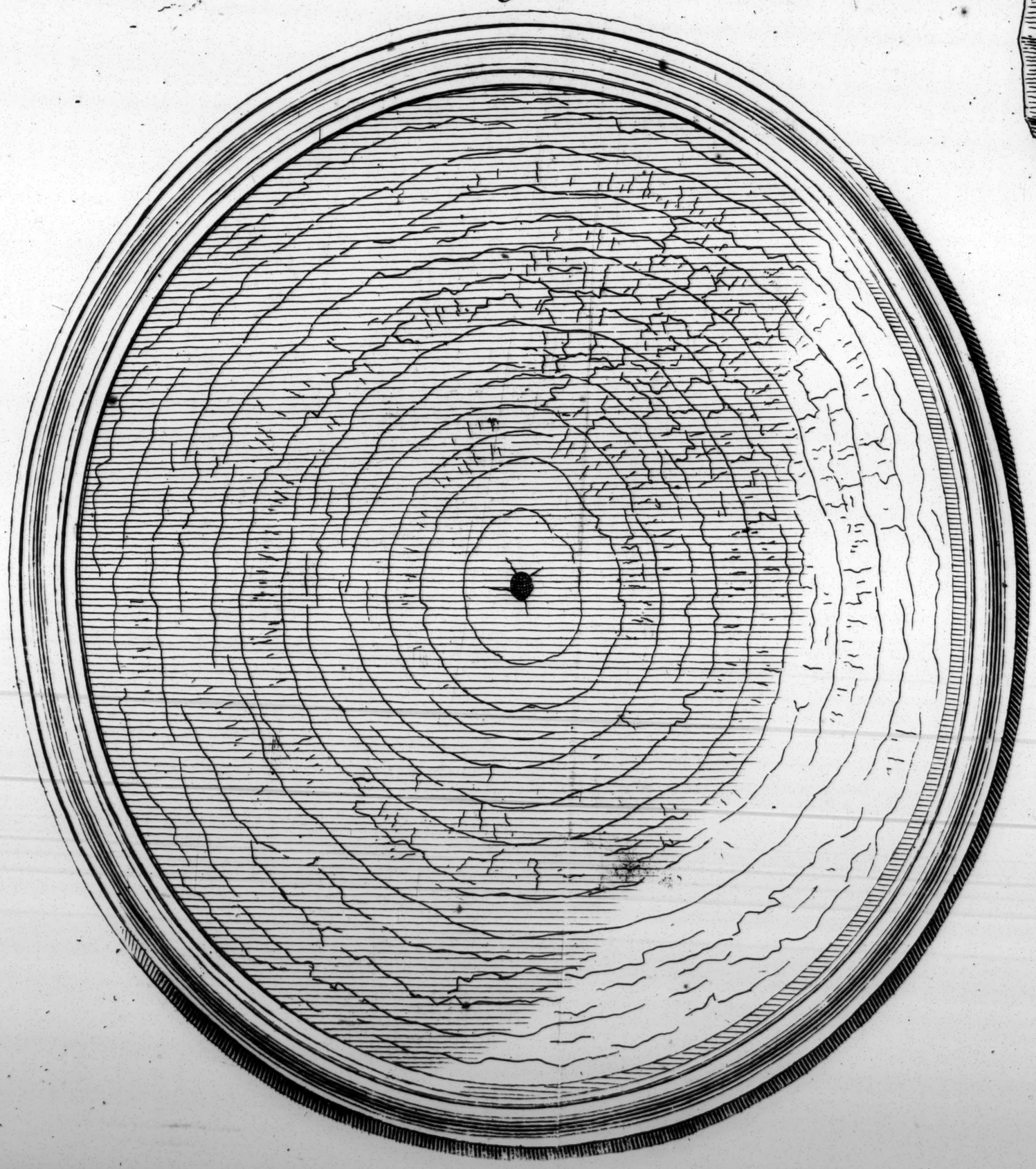


Fig. 3.



I. *An Account of* Elephants Teeth *and* Bones *found under Ground. By Sir* Hans Sloane, *Bart.*

IT is obſervable, that among the vaſt Variety of extraneous Subſtances lodged and found in ſeveral Layers of the Earth, at conſiderable Depths, where it is impoſſible that they ſhould have been bred, there are not ſo many Productions of the Earth, as of the Sea. And again, among thoſe which muſt have originally belonged to the Earth, there are many more remains of Vegetables, than of Land Animals. It appears, however, by the Hiſtories of paſt Times, and the Accounts of many, both antient and modern Authors, that Bones, Teeth, nay ſometimes very near entire Skeletons of Men and Animals have been dug up in all Ages of which we have Hiſtories, and almoſt in all Parts of the World, whereof thoſe, which were the moſt remarkable for their unuſual Size, have been alſo the moſt taken Notice of. Thus, for Inſtance, they have found in *Ireland*, the Horns, Bones, and almoſt entire Skeletons of a very large Sort of Deer, which is commonly believed to have been the Mouſe-Deer, an Animal of an uncommon Size, ſome of which Kind are thought to be ſtill alive in ſome remote and unfrequented Parts of the Continent of *America*. I ſhall in this Paper confine my ſelf chiefly to the Elephant, and ſuch Bones, *dentes exerti*, Tusks and Teeth of this Animal, as are either in my own Poſſeſſion, or have been mentioned by Authors I have

met with, to have been found under Ground. And firſt, as to thoſe Foſſile Teeth in my own Collecti. on, which unqueſtionably once belonged to Elephants, I ſhall here produce the following.

N° 116 of my Catalogue of Quadrupeds and their Parts, is the *dens exertus* of an Elephant, which was taken up, 12 Foot deep, from among Sand, or Loom, as they were digging for Gravel by the End of *Gray*'s-*Inn-Lane*, near *London*, and preſerved with tying it about with Whale-bones and Tape, to keep it from falling to Pieces, by Mr. *Conyers*, an ingenious Apothecary, and a great Collector of Curioſities of all Kinds.

As the greateſt Part of this Tooth was fallen to Pieces, nothing could be determined about its Length, when entire. The largeſt Piece, and alſo the moſt entire, hath five Inches and $\frac{8}{10}$ in Length, and 9 Inches and $\frac{6}{10}$ in Circumference, conſequently ſomething more than 3 Inches in Diameter. This Piece belonged to the Baſis, or Bottom of the Tooth; I mean, that Part by which it is articulated with the Head, as appears by a Cavity in form of a Cone, which all theſe Tusks have at Bottom, and which was filled, in this, with the Sand of the Gravel-pit wherein it was found.

The Condition this Tooth was found in, ſuggeſts the two following Remarks. It ſhews in the firſt Place, how far the ſubterraneous Steams are apt to calcine Subſtances of this Kind, which was done in this Tooth to ſuch a Degree, that it was grown extream brittle, and ready to fall to Pieces, and had moreover acquired an aſtringent Quality common to calcined Subſtances of this Kind, which makes them ſtick pretty cloſe, when held to the Tongue. They had altogether the ſame effect on the

the very large Skeleton, found near *Drapani* in *Sicily*, and mentioned by *Boccatius*, on that remarkable one found near *Tonna*, which hath been deſcribed by *Tentzelius*; as alſo on two Teeth found in *Northamptonſhire*, which I ſhall next take into conſideration. However it doth by no means follow from thence, that all Teeth and Subſtances of this Kind undergo the like Calcination by lying long under Ground, foraſmuch as there are others, as thoſe found in *Iſland*, and ſent to *Thomas Bartholin*, which were turned to a perfect hard, flinty Subſtance. It ſerves, in the ſecond Place, to aſcertain the Structure of theſe Teeth, and conſequently of Ivory in general, to be Layer upon Layer, or Coat upon Coat, like the Skins in an Onion, or rather the annual Circles, or Rings in Trunks of Trees. That this Tooth is compoſed of different Coats, ſurrounding and placed upon each other, is very apparent by the largeſt Piece remaining (Fig. 1.). I have already obſerved, that this Piece belonged to the Baſis of the Tooth, and there appear in it very viſible marks of nine Coats, ſome whereof have about one tenth of an Inch in thickneſs. Towards the further End of the Tooth, where it tapers almoſt into a Point, theſe ſeveral Coats alſo join together into two or three, and thoſe pretty conſiderably thick (Fig. 2.). With ſome Care theſe Coats might be farther ſub-divided into a conſiderable Number of other ſmaller ones, perhaps no thicker than a common Parchment. Farther, the very manner of its falling to pieces is an evident Proof of its Structure, all the Fragments being concave within, and convex without, and the Lines of Convexity and Concavity, Fragments of concentrick Circles, which the ſeveral Coats compoſed, when entire. *Thomas Bar-*

tholin, in his Treatiſe *De Unicornu* *, takes Notice, that Part of a foſſil *Unicorn*'s *Horn* having been calcined by Order of *Chriſtian* IV. King of *Denmark*, it was found to be compoſed, after the ſame manner, of thin Layers upon Layers; whence he infers, that it was not the Horn of an Animal, as was commonly pretended, but a Tooth, and namely the Tooth of a Sort of Whale in the Northern Seas, called *Narwhal*, as he had afterwards an excellent Opportunity to verify by one of theſe Unicorn's Horns ſtill ſticking in the Skull of the Creature, which was ſent to *Wormius* by *Thorlacus Scutonius*, Biſhop of *Iſland*. Nor is this Structure by any means to be looked upon as an Effect of the Calcination, whither brought about by the ſubterranean Steams, or by a chymical Trial, but is natural to the Tooth, as appears in ſome meaſure by a Piece of Ivory, marked 1181 (Fig. 3.); but ſtill more plain in another marked 731, where ſeveral of theſe Coats are by ſome Diſeaſe in the Tooth actually ſeparated from each other, like the Leaves of a Parchment Book, the Ivory on the other Side being ſtill firm and cloſe (Fig. 4.). This Structure appears likewiſe from the Teeth of the very young Elephant which died at *London*, where the uppermoſt Coat, being very moiſt, cracked upon drying, and broke at the Top.

N° 750, is Part of another *dens exertus*, which I had from the Reverend Mr. *Morton*, who in his Natural Hiſtory of *Northamptonſhire* †, gives the following Account of it: *An extraordinary Elephant's Tooth, one of thoſe which grow out of the upper Jaw, and which for their Magnitude and Length,*

* De Unicornu obſervationes novæ, *pag.* 102. † *Pag.* 252.

have

have by ſome Writers been accounted Horns, was lately taken out of the Earth by digging in Bowdon-parva *Field. Even the native Colour of it hath been in great meaſure preſerved; but it is become brittle with lying in the Earth; and was broken into three or four Pieces tranſverſely by the Diggers in taking it up. The two larger Pieces of it, which happily came into Mr.* Haldford's *Hands, were preſented to me. One of them is ſomewhat above a Yard; the other is two Foot in Length; but the whole Tooth muſt needs have been at leaſt ſix Foot long;—the thickeſt Part of the biggeſt Piece in my Poſſeſſion is ſixteen Inches round. The Tooth lay buried above five Foot deep in the Earth. The Strata from the Surface downwards to the Place where the Tooth was lodged, were as follows:* 1. *The Soil* 13 *or* 14 *Inches.* 2. *Loam, a Foot and a Half.* 3. *Large Pebbles, with a ſmall mixture of Earth amongſt them, two Foot and a Half.* 4. *Blue Clay. In the upper Part of this Stratum the Tooth was found.* Thus far Mr. *Morton.* That Part of this Tooth, which is now in my Hands, bears again very viſible Marks both of the Calcination it underwent by lying in the Earth, and of its laminated Structure (Fig. 5.).

N° 1185, is the *dens exertus*, or Tusk of an Elephant, remarkable for its large Size, and for its being ſo very entire. It was found under Ground in *Siberia*, and was brought from thence and given to me by Mr. *Bell*, an ingenious Surgeon, who was preſented with it by the Governor's Lady, in Lieu of a Reward for having cured her of a Diſtemper, upon his travelling through the Country in his paſſage with the Caravan ſent to *China*. It is very entire, of a browniſh Colour,

Colour, and hollow at Bottom like other Elephants Teeth, one of which it plainly appears to be. From the Baſis, meaſuring along the outward Circumference to the ſmall End, it is 5 Foot 7 Inches long, and along the inward Circumference 4 Foot 10 Inches. Meaſuring from the Inſide of the Baſis to the ſmall End in a ſtreight Line, the Diſtance is of 3 Foot 10 Inches and a Half. At the Baſis, where thickeſt, it meaſures one Foot ſix Inches round, and is there ſix Inches in Diameter: It weighs 42 Pound. The like Tusks, and other Bones of the ſame Animal, that is, of the Elephant, are found in ſundry Parts of *Siberia* to a conſiderable Quantity, and the Tusks and Teeth in particular, when leſs corrupted, are uſed all over *Ruſſia* for Ivory. *Henricus Wilhelmus Ludolfus*, in the Appendix to his *Ruſſian Grammar* *, mentions them among the Minerals of *Ruſſia*, by the Name of *Mammotovoikoſt*, and takes Notice, that the *Ruſſians* believe them to be the Teeth and Bones of an Animal living under Ground, larger than any one of thoſe above Ground. They uſe it in Phyſick in Lieu, and for the ſame Purpoſes with the Unicorn's Horn; and *Ludolfus* himſelf having been preſented with a Piece by one of his Friends, who ſaid, He had it from a *Ruſſian* of great Quality, lately returned from *Siberia*, found it to be true Ivory. He adds, That the moſt ſenſible among the *Ruſſians* affirm them to be Elephants Teeth brought thither at the Time of the Deluge. The Deſcription of theſe Teeth and Bones given by *E. Ysbrants Ides* †, is ſtill more extenſive, and withal ſo particular, that his whole Paſſage deſerves to be tranſcribed at Length. *Amongſt the Hills*, ſays he,

* *Pag.* 92. † In his Travels from *Moſco* to *China*.

which

which are ſituate to the North-Eaſt of Makofskoi, *not far from thence, the* Mammut's *Tongues and Legs are found; as they are alſo particularly on the Shores of the Rivers* Jenize, Trugan, Mongainſea, Lena, *and near* Jakutskoi, *to as far as the frozen Sea. In the Spring, when the Ice of this River breaks, it is driven in ſuch vaſt Quantities, and with ſuch Force by the high ſwollen Waters, that it frequently carries very high Banks before it, and breaks off the Tops of Hills, which falling down, diſcover theſe Animals whole, or their Teeth only almoſt frozen to the Earth, which thaw by Degrees. I had a Perſon with me to* China, *who annually went out in Search of theſe Bones: He told me, as a certain Truth, That he and his Companions found a Head of one of theſe Animals, which was diſcovered by the Fall of ſuch a frozen Piece of Earth. As ſoon as he opened it, he found the greateſt Part of the Fleſh rotten, but it was not without difficulty, that they broke out his Teeth, which were placed before his Mouth, as thoſe of the Elephant are; they alſo took ſome Bones out of his Head, and afterwards came to his fore Foot, which they cut off, and carried Part of it to the City of* Trugan, *the Circumference of it being as large as that of the waſte of an ordinary Man. The Bones of the Head appeared ſomewhat red, as though they were tinctured with Blood. Concerning this Animal there are very different Reports. The Heathens of* Jakuti, Tunguſi, *and* Oſtiacki, *ſay, That they continually, or at leaſt by reaſon of the very hard Froſts, moſtly live under Ground, where they go backwards and forwards; to con-*

firm which, they tell us, That they have often ſeen the Earth heaved up, when one of theſe Beaſts was on the March, and after he was paſt the Place, ſink in, and thereby make a deep Pit. They further believe, that if this Animal comes ſo near the Surface of the frozen Earth, as to ſmell, or diſcern the Air, he immediately dies, which they ſay is the Reaſon that ſeveral of them are found dead on the high Banks of the River, where they unawares come out of the Ground. This is the Opinion of the Infidels concerning theſe Beaſts, which are never ſeen. But the old Siberian Ruſſians *affirm, that the* Mammuth *is very like the Elephant, with this only Difference, that the Teeth of the former are firmer, and not ſo ſtraight as thoſe of the latter. They alſo are of Opinion, that there were Elephants in this Country before the Deluge, when this Climate was warmer, and that their drowned Bodies floating on the Surface of the Water of that Flood, were at laſt waſhed and forced into ſubterranean Cavities: But that after this* Noachian *Deluge, the Air, which was before warm, was changed to cold, and that theſe Bones have lain frozen in the Earth ever ſince, and ſo are preſerved from Putrefaction till they thaw and come to Light, which is no very unreaſonable Conjecture; though it is not abſolutely neceſſary that this Climate ſhould have been warmer before the Flood, ſince the Carcaſſes of drowned Elephants were very likely to float from other Places ſeveral hundred Miles diſtant, to this Country, in the great Deluge which covered the Surface of the whole Earth. Some of theſe Teeth, which doubtleſs have lain the whole Summer on the*

Shoar

Shore, are entirely black and broken, and can never be reſtored to their former Condition, but thoſe which are found in good Caſe, are as good as Ivory, and are accordingly tranſported to all Parts of Muſcovy. *The above-mentioned Perſon alſo told me, That he once found two Teeth in one Head, that weighed above* 12 Ruſſian *Pounds, which amounts to* 400 German *Pounds; ſo that theſe Animals muſt be of Neceſſity very large, though a great many leſſer Teeth are found. By all that I could gather from the Heathens, there is no Perſon ever ſaw one of theſe Beaſts alive, or can give any Account of its Shape.* Thus far *E. Ysbrant Ides.* What he obſerves of thoſe Teeth that are black and broken, may ſerve as a Comment to the following Paſſage of *Pliny* *: *Theophraſtus autor eſt, & ebur foſſile candido & nigro colore inveniri, & oſſa è terra naſci, invenirique lapides oſſeos.* *Lawrence Lang*, in the Journal of his Travels to *China*, whither he went with Diſpatches from His *Czariſh* Majeſty in 1715, takes Notice of theſe Bones †, as being found about the River *Jeniſei*, and towards *Mangaſea*, along the Banks, and in the Hollows occaſioned by the Fall of the Earth. He calls them *Maman-bones*, and informs us, that ſome of the Inhabitants are of Opinion, that they are no real Bones, Teeth, *&c.* but a Sort of *Cornu Foſſile*, that grows in the Earth, and that others will have them to be the Bones of the *Behemoth* mentioned in the 40th Chapter of *Job*, the Deſcription whereof they pretend fits the Nature of the Beaſt, whoſe Bones and Teeth they are imagined to be, thoſe ſuppoſed Words, in particular, that he *is caught with*

* Lib. xxxvi. C. 18. † Preſent State of *Ruſſia*, Vol. II. *pag.* 14.

his own Eyes, agreeing with the *Siberian* Tradition, that the *Maman* Beaſt dies upon coming to Light. The ſame Author affirms, from the Report, as he ſays, of credible People, That there have been ſometimes found Horns, Jaw-bones and Ribs, with freſh Fleſh and Blood ſticking to them. The ſame is confirmed by *John Bernard Muller*, in his Account of the *Oſtiacks* *, who adds, That the Horns in particular have been found ſometimes *all bloody at the broken End, which is generally hollow, and filled with a Matter like concreted Blood*; that they find, together with theſe Teeth, or Horns, as he calls them, the Skull and Jaw-bones with the Grinders ſtill ſticking in them, all of a monſtrous Size; and that he himſelf, with ſome of his Friends, hath ſeen a Grinder weighing four and twenty Pounds, and better; that the Inhabitants make divers Sort of Works of theſe Teeth, and that they are moſtly to be met with in the coldeſt Places of *Siberia*, as for Inſtance, *Jakutsky*, *Bereſowa*, *Mangaſea*, and *Obder*. He likewiſe gives the Deſcription of one of theſe Animals, from the Accounts of ſeveral Perſons, who aſſured him, That they had ſeen them in the Caverns of the high Mountains beyond *Bereſowa*: But as this Deſcription hath very much the Face of a Fable, I forbear inſerting it here. The Author of the *Preſent State of* Ruſſia † obſerves, that ſome of the *Swediſh* Priſoners baniſhed into *Siberia*, got their Livelihood by turning Snuff-boxes out of theſe Teeth; and in another Place ‡ he mentions them among the *Siberian* Commodities, of which the Czar hath the Monopoly.

* Ibid. *pag.* 52. † Vol. I. *pag.* 12. ‡ *Pag.* 78.

The

The Accounts which I have hitherto given of theſe *Maman*-bones and Teeth, or at leaſt their moſt eſſential Parts, are confirmed by a Letter of *Baſilius Tatiſchow*, Director General of the Mines in *Siberia*, and Counſellor of the Czar's Metallick Council, wrote to the learned *Ericus Benzelius*, now Biſhop of *Gothenburg*, and printed in the *Acta Literaria Sueciæ* (M.DCC.XXV. *Trimeſtre Secundum*, pag. 36.) wherein he mentions the following Pieces he had in his own Poſſeſſion: A large Horn, as he calls it, or Tooth, weighing 183 Pounds, which he had the Honour to preſent to his *Czariſh* Majeſty, and is now kept in the Czar's Collection of Curioſities at *Petersburg*; another large Horn, which he preſented to the Imperial Academy at *Petersburg*; another ſtill larger than either of theſe two, which he cauſed to be cut, and carved himſelf ſeveral Things of it, the Ivory being very good; Part of the Skull, corrupted by having lain in the Ground, and ſo large, that it ſeemed to him to be of the ſame Size with the Skull of a great Elephant; the Forehead in particular was very thick, and had an Excreſcence on each Side, where the Horns uſually ſtick to it, which Excreſcence however, as the Author obſerves, was ſo ſmall, as to make him doubtful, whether or no there was ever any Horns ſtuck to them. The Cavity, wherein the Brain was lodged, was exceedingly ſmall in Proportion to the Bulk of the Skull. He had found alſo a ſpungy Bone of a Foot and a Half in Length, and three Inches in Breadth, ſticking to the Skull, and of a conical Figure, whence he conjectured, that it ſerved to ſupport one of the Horns, which is obſerved alſo in other Animals that bear Horns: Laſtly a Grinder, which had

ten Inches in Length, and ſix Inches in Breadth, beſides ſeveral of the Ribs, Shank-bones, and other Bones found from Time to Time, which the Author forbore mentioning. The ſame Author hath taken no ſmall Pains to inquire into the true State of thoſe Pits and Hollows which the Pagan Inhabitants of *Siberia* ſay, theſe Animals make when they walk under Ground, and found that they were nothing but Caverns, ſuch as are common in other mountainous Countries, and are owing to the Force of ſubterranean Rivers and Cataracts, which at laſt eat through and undermine the Places where they paſs, ſo as to make the Ground above them give Way and ſink in. This is what I found remarkable in this Letter of Mr. *Tatiſchow*. I cannot forbear adding, that although the Author hath left the grand Queſtion about the Origin of theſe Bones undetermined, yet his Obſervations ſeem to me to contribute very much to eſtabliſh the Opinion above related, that theſe Bones are the Bones, and the Horns, as he calls them, the Tusks of Elephants drowned in the univerſal Deluge. It is to be hoped, that this Matter will one Time or other be ſet into a ſtill clearer Light, particularly after the Order his late *Czariſh* Majeſty was pleaſed to give to the Governor General of *Siberia*, to ſpare no Care nor Coſt to find a whole Skeleton of this Animal, and to ſend it to *Tatiſchow*.

Before I proceed farther, I will beg Leave to add one Obſervation of *Cornelius le Brun*, who in his Travels through *Ruſſia* to the *Eaſt-Indies*, tells us, That the Neighbourhood of *Veronitz* they had found ſeveral Elephants Teeth on the Surface of the Ground, which no Body could tell how they came thither

thither, and that the Czar's Opinion about them was, that *Alexander* the Great, when he paſſed the *Tanais*, or *Don*, advanced as far as *Koſtinka*, a ſmall Town eight Werſts from thence, and that probably ſome of his Elephants died there, of which thoſe Teeth were the Remains.

N° 764 of my Collection, is one of the Grinders of an Elephant, which was likewiſe found in *Northampton-ſhire*, for the Deſcription whereof I ſhall again borrow the Reverend Mr. *Morton*'s own Words *. *Northwards*, ſays he, *about fifty Yards from this Place*, (where the above-mentioned *dens exertus* was found) *was alſo digged up one of the Molares, or Grinder-teeth of an Elephant, perhaps of the ſame that the Tusk belonged to. The Grinder whole, or however all the Pieces of it I could find (for it was broken into three or four in taking it up) being put together as they grew, exhibit* 13 *or* 14 *parallel Lamellæ; each of which extends the whole Length, and almoſt the whole Thickneſs of the Tooth; and of theſe for the Main it is compoſed. But in a live, or perfect Tooth, theſe Lamellæ do not appear ſo plainly, being in Part cruſted over with a white oſſeous Cruſt, or Integument, which in this Foſſil Tooth is almoſt wholly periſhed and gone, inſomuch that the Lamellæ are more expoſed to View. From the Root to the Top in the longeſt Part, which is near the Middle of it, it is juſt ſeven Inches long. Its Thickneſs in the thickeſt Part of the Root, which is alſo near the Middle of it, is almoſt three Inches, and it is a little above eight Inches broad: Meaſuring it this*

* Natural Hiſtory of *Northamptonſhire*, C. iii. §. cxxxv. *pag.* 252.

Way,

Way, we take in the whole Pile of the Lamellæ. None of the Lamellæ are contiguous; there interpoſes betwixt them a thinner Plate of a whiter Colour, and a laxer Texture. Three or four of the outmoſt at one End of the Pile, appear undulated at the Top of the Tooth, are near as broad at Top as at the Root, and have a blunt Ending. The reſt of them are by Degrees contracted to a Point, are gradually ſhorter and ſhorter to the other Extremity of the Pile, and alſo bend a little over one another. And each of them, as it approaches the Top, divides, as it were, into ſeveral ſmaller Teeth; and with theſe the Lamellæ of this Figure terminate. The above-deſcribed Tooth was lodged at almoſt twelve Foot Depth in the Earth. Above it were the following Strata; 1. The top Earth, a blackiſh, clayey Soil, about 16 Inches. 2. Sandy Clay intermixed with Pebbles, five Foot. 3. A blackiſh Sand with ſmall white Stones in it, one Foot. 4. A loamy, ſofter Sort of Gravel, one Foot. 5. A ſharper Gravel, about two Foot. The Tooth was found a Foot and a Half deep in this Stratum of Gravel. Below this fifth Stratum there was a blue Clay. Thus far Mr. *Morton*. It is very viſible, that this Grinder alſo, by lying in the Earth, hath undergone the ſame Alteration with the Tusk above deſcribed found in *Bowdon-parva* Field.

N° 119, and 120, of my Catalogue, are two Pieces of another large Grinder, very probably of an Elephant too, turned to a very hard, ſtony, and almoſt metallick Subſtance.

N° 121 is a Piece of the *Molaris*, or Grinder of an Elephant, where the undulated Lamellæ are ſet very cloſe to each other.

N°

N° 122 is a Piece of another Grinder, perhaps of an Elephant. It hath very apparent Marks of being foſſile, as well as the preceding, and is farther remarkable, for that a petrifying Subſtance being got between the Lamellæ hath very conſiderably ſeparated and divided them from each other, in ſuch a Manner, that they appear to have been ſet very looſe.

N° 427, of my Collection of Quadrupeds and their Parts, is Part of an Elephant's Skull which was found at *Glouceſter* after the Year 1630, together with ſome large Teeth, ſome five, others ſeven Inches in Compaſs, according to a ſhort Inſcription wrote upon this very Piece.

N. B. Tab. 11. Fig. 7. is one of the Tusks of the young Elephant that died at *London*, of which Mention hath been made above.

II. *Obſervationes Aſtronomicæ à R. P.* Joh. Baptiſta Carbone *tranſmiſſæ, communicante* Iſ. de Seguera Samuda, *M. D. R. S. S. & Coll. Med.* Lond. *Lic.*

Obſervatio Solaris Eclipſis propè Ulyſſiponem habita *die* 15 Septembris *Manè* 1727. N. S. *à P.* Joanne Baptiſta Carbone S. J.

IN *Prædio*, quod eſt occidentalius noſtro Collegio D. Antonii M. 4″ hor. circiter, & cujus Latitudo Quadrante aſtronomico trium pedum explorata, eſt 38° 42′ 58″, obſervavi hanc Eclipſim Teleſcopio pedum circiter 8, quod micrometro inſtruxeram vite comparato. Initium infra horizontem celebratum eſt; jamque digitos circiter 4 deficiebat Sol, quando ex oppoſito monte primò emerſit. Sequentes tamen phaſes

obſervari

obſervari tantùm potuere, reliquis fortuito eventu impeditis.

Digiti		H.	′	″	
	Immerſ.	*Temp.*	*Ver.*	*corr.*	
VI ½	— — — —	5	55	8.	*dub.*
VIII.	— — — —	6	10	54.	*dub.*
VIII,	m. i′, ſec. 48″ max. obſc.	6	13	29.	*circ.*
	Emerſ.				
VI ½	— — —	6	31	49.	
VI	— — —	6	35	23.	
V ½	— — —	6	38	45	
V	— — — —	6	41	57	
IV ½	— — —	6	45	2	
IV	— — —	6	47	59	
III ½	— — —	6	50	49	
III	— — —	6	53	34	
II ½	— — —	6	56	16	
II	— — — —	6	58	54	
I ½	— — —	7	1	28	
I	— — —	7	3	59	
½	— — —	7	6	28	
Finis Eclipſis	— —	7	9	2.	*certiſſ.*

Poſt finem Eclipſeos, ſtatim horologium pendulo inſtructum, quo ad temporis dimenſionem uſus ſum, duplici Solis altitudine eodem quadrante aſtronomico ſucceſſivè obſervatâ, ad trutinam revocavi; inventamque correctionem, in phaſibus ſuperiùs adnotatis adhibui.

Octobris die 15, Immerſionem intimi Jovis Satellitis obſervavi, teleſcop. ped. 22. Contigit autem H. 9 10′ 54″.

Novem-

Novembris 7, ejuſdem Satellitis Immerſ. eodem teleſcopio, *H.* 9. 25′ 45″.

Obſervationes Romæ *habitæ in Eclipſi Solis, Manè conſpecta eodem die ad Radices* Collis Quirinalis.

Horæ. Minuta. Secunda Temporis veri poſt mediam noctem ex tranſitu Solis rectificatur.

H. ′ ″

7. 0. 0. INtendebam ad conſpectum Sirij verſantis in vicinia meridiani; cum ex Ephemeridum calculis poſt aliquot minuta Eclipſeos initium promitteretur. Interea tamen directo in Solem tubo optico, exceptaque in charta imagine, jam cæpiſſe video Eclipſim ex aliquot minutis horariis. Videram die præcedenti maculas plures apparere in diſco Solis: Quas cum in figura ſuis locis reddere cuperem ante initium Eclipſeos, ne differem poſt earumdem contectionem à diſco Lunæ commenſum illarum, ac ſitum reſpectu azimuthi adnotare; huic deſcriptioni ſtatim incumbendum duxi, adeoque phaſes Eclipſeos priores attendere prætermiſi. Completa celeriter macularum delineatione ſequentes phaſes accurate adnotavi cum V. C. Jo. Dominico Maraldi, uno verò ex meis domeſticis numerante horaria ſecunda temporis clarâ voce in horologio pendulo inſtructo, & pluribus diebus ante ad meridiem exacto, ad phaſes ſingulas. (*Vid. Tab.* 3.)

7. 2. 17. Latent Solaris diſci digiti tres cum quadrante à Luna contecti. Interſectio diſcorum

Solis, & Lunæ incidit in gr. 5. & 95. numeratis à puncto, quod eſt in figura inverſa verticale, nempe *A*. Macula *a* eſt in plano azimuthi per centrum Solis à Zenith ducti, & diſtat in ſemidiametro diſci Solaris à centro verſus peripheriam digitis 4 $\frac{1}{4}$, ut in figura. Etiam macula *e* in eodem ferè

H. ′ ″ Azimutho verſatur.

7. 21. 47. Cum per aliquod temporis intervallum nubes intercepiſſent adſpectum Solis, paulo ante reſtituta ſerenitate, nunc ſpectantur à Luna contecti digiti Solaris diſci 4 $\frac{1}{2}$. Interſectio diſcorum Solis, & Lunæ fit in gr. diſci Solaris 10. & 111. ut antea à puncto *A* imaginis per Lævam reſpicientis numeratis.

7. 24. 0. Veſtigium dilutionis maculæ *m* prope *b* ſitæ Lunæ diſcum ſubit.

7. 24. 40. Macula *b* incipit perſtringi à diſco Lunæ.

7. 25. 11. Eadem macula *b* tota immergitur.

7. 27. 41. Initium maculæ *c* incipit ſubire diſcum Lunæ.

7. 28. 31. Eadem macula *c* tota jam occultatur.

7. 29. 10. Digiti Solaris diametri 5 $\frac{1}{2}$ latent.

7. 31. 9. Latent digiti 5 $\frac{1}{4}$. Interſectio diſcorum utriuſque Luminaris fit in gr. 20, & 136.

7. 38. 45. Latent digiti 6, & gr. 31. atque 150 Solaris diſci ſunt puncta interſectionum cum Lunari.

7. 40. 58. Macula *d* Limbus prior à Lunæ circulo perſtringitur.

7. 41. 45. Tota macula *d* jam latet.

7. 43. 15. Conteguntur Solis digiti 6 $\frac{1}{4}$; & interſectio diſcorum incidit in gr. 39. & 162.

H. ′ ″

7. 45. 26. Maculæ fermie Evanidæ *f* Limbus perſtringitur ab incurſu Lunæ.

7. 46. 20. Tota macula *f* à Luna contegitur.

7. 50. 0. Latent digiti 6 ½, & luminarium peripheria ſe interſecant in gr. 61. & 185.

8. 0. 12. Latent paulo minus quàm digiti ſex, ſeu 5 $\frac{7}{8}$, interſecant ſeſe diſci in gr. 62. & 182.

8. 2. 25. Latent digiti 5 ¼ ex gr. 63. ad 183.

8. 5. 24. Latent digiti 5 ¼. Interſectio Luminarium diſci in gr. Solaris 80. & 192.

8. 8. 32. Latent digiti 5. Interſectio in gr. 82 ½, & 192 ½.

8. 11. 50. Incipit emergere Limbus prior maculæ *b*.

8. 12. 38. Tota macula *b* extra Limbum Lunæ; & macula *n* eidem proxima ſimul exit.

8. 14. 46. Emergit etiam macula *m* vicina maculæ *b*.

8. 16. 34. Emergit quoque macula *l* eidem *b* proxima.

8. 18. 29. Macula *c* emergit.

8. 22. 38. Latent digiti 3 ½. Interſectio diſcorum in gr. 105. & 195.

8. 23. 40. Incipit emergere macula *d*.

8. 24. 10. Tota macula *d* extra diſcum Lunæ.

8. 27. 23. Latent digiti 2 ¼. Interſectio diſcorum fit in gr. Solaris 115. & 182.

8. 34. 5. Incipit emergere è diſco Lunæ prior limbus maculæ *e*.

8. 34. 55. Tota macula *e* extra limbum Lunæ.

8. 35. 46. Latent digiti 1 ½.

8. 37. 9. Latent digiti 1 ¼.

8. 37. 27. Exeunt extra Limbum Lunæ maculæ *g*, & *h*, ſitæ prope maculam *e*.

H.	′	″	
8.	39.	46.	Latent digiti o ¼. Interſectio Limborum utriuſque diſci Luminarium in Solari incidit in gr. 140. & 180.
8.	42.	8.	Limbus ſuperior Solis diſtat à Vertice gr. 58. 1′. inſpectus per quadrantem aurichalchicum tubo inſtructum, cujus radius eſt palm. Rom. 3.
8.	44.	10.	Finis Eclipſeos, nunc primum excedente omnimodè ex Limbo Solis Lunæ diſci extremo.
8.	46.	53.	Limbus ſuperior Solis per quadrantem aurichalchicum inſpectus diſtat à vertice gr. 57. 30′. adeoque centrum Solis diſtat à vertice gr. 57. 46′.
8.	48.	1.	Limbus ſuperior Solis iterum inſpectus diſtat à vertice gr. 57. 20. adeoque centrum Solis diſtat à vertice gr. 57. 36′.
			Limbus inferior Solis diſtat à vertice per ſupradictum quadrantem gr. 57. 20′. adeoque centrum Solis diſtat à vertice gr. 57. 4′.
			Eadem die in meridie cœlo clariſſimo
11.	58.	25.	Primus Solis limbus cum penumbra attingit lineam meridianam.
0.	1.	35.	Secundus Solis Limbus cum penumbra attingit Lineam meridianam.

	H.	′	″
Ergo Meridies —	0.	0.	0.

In meridie diſtantia à vertice per quadrantem explorata fuit

Gr. 38. 27′. Limbi ſuperioris. } Centri Solis
Gr. 38. 59′. Limbi inferioris. } gr. 38. 43′.

Obſervatio

Obſervatio Solaris deliquii die 14 *Septembris* 1727. N. S. *habita in Obſervatorio* Bononienſi *à Cl.* Euſtachio Manfredi.

PLurimæ in Sole maculæ hujuſce Eclipſis tempore videbantur: Cùm tamen præ nubibus neceſſariæ circa illas inſtitui obſervationes non potuerint, ut earum ſitus in Solari diſco deprehenderetur, præcipuarum tantùm aſpectus in appoſita figura exhibetur, qualis inferri potuit ex obſervationibus veſpere diei 14. circa hor. 5. 26′. habitis; idque ſatis eſſe arbitror ad eas indicandas quæ ſunt à Luna occultatæ Eclipſis tempore. Notandum tamen, prædictas maculas, ex quo obſervatæ ſunt veſpere diei 14. uſque ad Eclipſis tempus, non modo ſingulas in ſuo parallelo Solari proceſſiſſe, quantum regularis motus Solis fert, verùm etiam pleraſque earum ſine lege immutatas apparuiſſe tum figurâ, tum magnitudine; nonnullas etiam in plures diviſas, aliaſque in unum compactas; alias demum in Limbo Solis orientalis viſas, quæ veſpere præcedenti non apparebant.

Initium Eclipſis obſervari non potuit; ſed hor. 18. 55′. 48″. Aſtronomic. ſenſibilis apparebat defectus, ut inter nubes.

H.	′	″	
18.	59.	37.	Unus digitus, & amplius fortaſſe latebat.
19.	3.	12.	Digitus cum dimidio.
19.	6.	50.	Duo digiti.
19.	10.	11.	Duo cum dimidio. *dub.* Paulò poſt nubes Solem obtegunt.
19.	30.	35.	Sol inter nubes videtur deficere plus digitis 4. cum dimidio.

Eclipſis

H. ′ ″

19. 35. 46. Eclipſis nondum pertingere videtur ad 5 digit.

19. 40. 47. Quinque digiti circiter.

19. 44. 31. Maculæ, *b*, *c*, *d*, *e*, jam latebant. Nunc occultatur centrum, *i*.

19. 47. 27. Limbus ‹ inter maculas, *s*.

19. 47. 46. Secunda ex duabus maculis, *s*, omnino tegitur.

19. 51. 37. Eclipſis eſt paulò minor 5 digit. inter nubes.

19. 54. 12. Centrum maculæ, *p*, occultatur.

19. 55. 37. Centrum, *q*, item occultatur.

19. 59. 2. Quatuor dig. cum dimidio eclipſantur. *dub.*

20. 1. 22. Una ex maculis prope limbum (fortaſſe *n*) occultatur.

20. 10. 10. Eclipſis paulò min. dig. 3 ½.

20. 11. 47. Tota macula *i*, emerſit.

20. 15. 0. Eclipſis 3 digitorum.

20. 18. 48. Duorum cum dimidio.

20. 18. 49. Incipit emergere macula, *s*.

20. 22. 26. Duo digiti circiter eclipſantur.

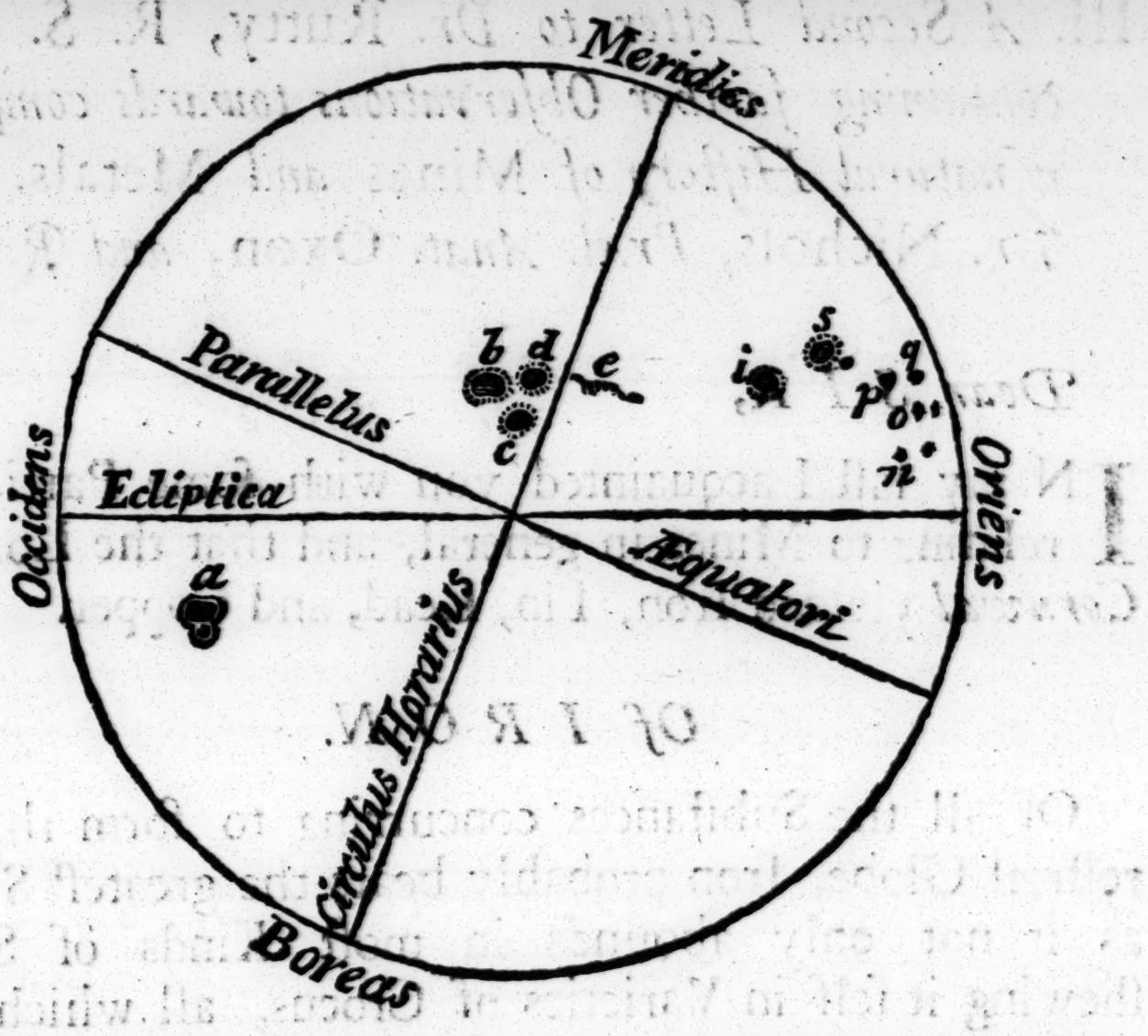
20. 26. 14. Digitus 1 ½.

20. 36. 6. Finis Eclipſeos a tribus obſervatoribus notatus, in eodem ſecundo temporis concordibus.

14. *Sep.*

14. *Septembris*, 1727, *hora* 5. *min.* 26.

Figura macularum Solarium inverſo ſitu.



Obſervatio defectus Solis habita Patavii XVIII. *Kal. Octobris* 1727. N. S. *a Comite* Joanne Poleno.

DEnſioribus tamen nubibus ſæpius intercurrentibus, non plures obſervari potuerunt Phaſes, quam eæ, quas ſubjeci.

Temp. Ver. H.	'	''		*Digit.*	'
19.	3.	45.		0	10
	24.	12.		3	0
	41.	27.		4	30
20.	30.	45.		1	30
	38.	42.		Finis.	

III. *A Second Letter to Dr.* Rutty, R. S. Secr. *containing farther Obſervations towards compoſing a natural Hiſtory of* Mines *and* Metals. *By Dr.* Nichols, *Præl. Anat.* Oxon, *and* R. S. S.

Dear S I R,

IN my laſt I acquainted you with ſome Particulars relating to Mines in general, and that the Loads in *Cornwal* yielded Iron, Tin, Lead, and Copper.

Of I R O N.

Of all the Subſtances concurring to form the terreſtrial Globe, Iron probably bears the greateſt Share; as it not only abounds in moſt Kinds of Stone, ſhewing it ſelf in Varieties of Crocus, all which gain a more intenſe Colour by Fire; but enters likewiſe greatly into the Compoſition of common Clay; as may be judged from the Similitude of Colour between Clay and dry Iron Oar; from the eaſy Vitrification of Clay; from the Reſemblance between Clay ſo vitrify'd and the Clinkers of Iron; from its deep red Colour after Calcination; and laſtly, from its yielding pure Iron, by being burned with Oil.

But while Iron is thus entangled with other Bodies, it rarely employs the Care of the Miner; who finds the Expence of reducing it to Metal too ſeldom ballanced by the Price it yields: For which Reaſon, tho' we frequently meet with large and rich Loads of Iron,

yet

yet (the Woods having been apply'd to more advantagious Uſes) they are there entirely neglected.

When it is moſt pure, I find the Oar under three different Appearances.

Paper the firſt contains a Piece of rich dry Iron Oar, whoſe Scrapings exactly reſemble an *Alkohol Martis:* This Kind of Iron Oar has very nearly the Colour of common Clay.

Paper the ſecond contains a Piece of rich Iron Oar, with Part of the Wall of the Load formed by a Concretion of yellow Cryſtals. In this Stone the Iron radiates from Points forming Segments of Spheres, and where theſe Spheres leave any Interſtices, you will find a Crocus, or Oker.

Paper the third contains a Stone of Iron of the Kind uſed for burniſhing Plate; it is of the Species of the *Hæmatites.*

Both theſe laſt Stones ſcrape into a deep Crocus.

From the ſecond Inſtance we may conjecture, that the yellow Colour in Cryſtals ariſes from a Crocus entangled with the ſtoney Salts.

Though the Want of Wood in *Cornwal* deprives it of the Advantages it might otherways reap from Iron as a Metal, we ſhall nevertheleſs find it far from being a uſeleſs Oar, when we conſider it as ſometimes impregnating the Waters with vitriolick Salts, thereby making them a proper Menſtruum for diſſolving the diſſeminated Particles of Metals; ſometimes deſtroying the ſulphureous Menſtrua, which (tho' they diſſolve the diſſeminated Metals) do nevertheleſs obſtruct their new Concretions; and ſometimes as being it ſelf the Magnet by which the metallick Particles are attracted into new Concretions.

Of T I N.

The next metallick Subſtance found in *Cornwal*, and from which theſe Iſlands are ſuppoſed to take their Name, is *Tin*. It is never found but as an Oar; whereas Gold is never found but as a Metal, at leaſt its Oar is unknown, and all other Metals are found ſometimes as a Metal, and ſometimes as an Oar.

Tin always ſhoots into Cryſtals which are of different Magnitudes from two Ounces in a ſingle Cryſtal to ſuch as eſcape our Sight. Theſe Cryſtals are for the moſt Part interſperſed in Loads of other Subſtances.

Paper the fourth contains Tin Cryſtals interſperſed in a Load of a Kind of Clay, in which is obſervable a conſiderable Quantity of Red-oker.

Paper the fifth contains a Stone of hard Iron-Stone, in which are exceeding ſmall Cryſtals of Tin.

Paper the ſixth contains ſomewhat larger Cryſtals, interſperſed in a dry Red-oker.

Paper the ſeventh contains Tin Cryſtals, interſperſed with Sparr-ſtone and a Sort of Marl.

Paper the eighth contains larger Cryſtals, interſperſed in a Kind of Clay and Red-oker, as in Paper the fourth.

When a hundred Sacks of the Load (each containing more than a *Winchester* Buſhel) yield one Gallon of clean Oar, the Load is eſteemed very well worth working.

Sometimes theſe Cryſtals are ſo collected into one Maſs, as to form Loads of pure Tin Oar, and ſo large as to yield to the Value of 100 *l.* every twenty-four Hours.

Paper the ninth contains two Stones of ſuch pure Loads, in which obſerve the one is black, and the other nearly white.

Theſe Cryſtals concrete ſometimes into the Form of a Parallelipepidon, whoſe Summit is covered by a Pyramid; ſometimes the Angles formed by the Sides of the Pyramid, and ſometimes the Summit of the Pyramid are as it were plained away.

Paper the tenth contains ſeveral of theſe Cryſtals, of which Number the firſt contains a whole Cryſtal, which has none of its Angles off. (*See Plate the 2d. Fig. the 8th.*). The ſecond contains a Cryſtal which has only two of its Angles plained away. (*Fig. the 9th.*). The third contains a Cryſtal which has all its Angles plained away. (*Fig. the 10th.*). The fourth contains a Cryſtal which has all its Angles and its Summit plained away. (*Fig. the 11th.*)

Sometimes the Cryſtals repreſent two equal pentelateral Pyramids joined at their Baſe,

As in Paper the eleventh, which contains two Cluſters of Cryſtals, which conſidered ſeparately are of that Form.

Under whatſoever Form theſe Cryſtals ſhoot, they always carry an exceeding fine Surface; which, when rubbed off, can be renewed by no Art. In Paper the fourth one Side of the Parallelipepidon is rubbed away, to ſhew its Appearance after loſing its natural Surface.

Theſe Cryſtals are of different Colours from the White (like white Sugar candy'd) to the deep Black. Thus Paper the twelfth contains a groupe of ſmall white Tin Cryſtals, which are very uncommon. Theſe white Cryſtals ſeem to me to carry a finer Luſtre than any other I ever ſaw, and are perfectly tranſparent; ſo

that were they found of equal Size with the black Cryſtals, and of a white Water (which I imagine may be) their Hardneſs and Weight (in both which they exceed any other Foſſil) would probably make them preferable to the Diamond. However, as the deeper Colours of theſe Cryſtals ſeems to ariſe from a greater Proportion of Iron in their Compoſition, which they throw off in an Iron Slag upon Fuſion, and which changes by proper Degrees of Heat into a Crocus, thereby changing the Colour of the Cryſtal to a brighter Red; ſo the white Tin Oar is certainly to be eſteemed both richeſt and beſt, as moſt free from Iron.

Paper the thirteenth contains a Piece of the Load contained in Paper the ſeventh, in which the Cryſtals are of a brighter red, from its being heated red-hot. (*Theſe Specimens were all produced before the Society, and are ſince preſented to Sir* Hans Sloane, *Preſident.*)

Theſe Cryſtals ſeem to be the heavieſt Bodies the Earth produces, except Quickſilver and actual Metals. Their ſpecifick Gravity is to Water as 90 $\frac{1}{2}$ to 10; to Rock Cryſtal in Water as 90 $\frac{1}{2}$ to 26; to Diamond as 90 $\frac{1}{2}$ to 34; and to pure malleable Tin, as found by repeated Trials, as 90 $\frac{1}{2}$ to 78; from whence appears the Poſſibility of what ſome Miners affirm, *viz.* That a cubical Inch of ſome Tin Oars, will yield more than a cubical Inch of Metal.

Having already taken Notice that the Cryſtals of Tin are ſometimes ſo ſmall as to eſcape the Eye, and ſo diſſeminated in the Load as not to make above $\frac{1}{800}$th, or $\frac{1}{1000}$ Part of the Load, one would naturally imagine it an endleſs Labour to cleanſe the Oar from ſuch a vaſt Diſproportion of Rubbiſh. But the great

ſpecifick Gravity of theſe Cryſtals renders the cleaning it leſs troubleſome, and leſs expenſive, than in any other Oar whatever. It requires no more, than that the whole Stuff be ſtamped to a fine Powder, after which it is waſhed by a Water, whoſe Force is ſo moderated as to waſh away only the lighteſt Parts. This Stamping and Waſhing is repeated till the Oar is left exceedingly clean, and yields in Metal from $\frac{18}{20}$ to $\frac{19}{20}$th, according as it is cleanſed from the Load, and as it is in its own Nature more or leſs free from Iron.

Begging Leave to defer the Account of *Lead* and *Copper*, I am

Your very humble Servant,

July the 1ſt. 1728.

F. Nicholls.

IV. *A Method of raiſing ſome exotick Seeds, which have been judged almoſt impoſſible to be raiſed in* England, *communicated in a Letter to Dr.* Douglas, *Coll. Med. Soc. honorar. and R. S. S. By Mr.* Philip Miller, *Gardiner to the Phyſick-Garden at* Chelſea.

SIR,

ACcording to my Promiſe, I here ſend you an Account of the Methods I have taken to raiſe the *Coco-Nut*, with the Succeſs of each; which hath led me

me to a ſure Method for raiſing ſuch Seeds which have hard Coats, or Shells ſurrounding them; and have been judged very difficult, if not impoſſible to be raiſed in *England*.

In the Year 1724, I had a Parcel of freſh *Coco-Nuts* given me, which were brought over from *Barbados*: Part of theſe Nuts I diveſted of their outer Coat, or Husk, and the other Part I left intire as I received them. Both theſe Parcels I planted in large Pots, filled with good freſh Earth, and plunged the Pots into a Hot-bed made with *Tanners-bark*; giving them gentle and frequent Waterings as the Earth in the Pots ſeemed to require; but had not one, out of the whole Number, which made any Attempt to ſhoot, as I could perceive; and upon taking them out of the Pots, I found they were rotten. About four Months after, I received another freſh Parcel of *Coco-Nuts* from *Barbados*, which I treated in another Manner: Part of theſe I cut off the outer Coat or Husk from, and the other Part I left intire as before: But ſuppoſing it was owing to my planting the other Parcel in *Pots*, that they did not ſucceed, I made a freſh Hot-bed (with *Horſe-dung*) and covered it over with *freſh Earth*, about 18 Inches thick, in which I planted the Nuts: obſerving as before, to ſupply it with *convenient Moiſture*, as alſo to keep the Hot-bed in an *equal Temper of Heat* (which I was guided to do by a *Thermometer* graduated for the Uſe of *Hot-beds*); but with all my Care I had no better Succeſs than before; not one of the Nuts making any Eſſay towards *ſhooting*. The Year following I had another Parcel of *Coco-Nuts* given me, which, conſidering my former ill Succeſs, I planted in a different Manner, as follows. Having a Hot-bed,

which

which had been lately made with *Tanners-bark*, and which was filled with Pots of exotic Plants, I removed two of the largeſt Pots, which were placed in the Middle of the Bed, and opening the *Tanners-bark* under the Place where the two Pots ſtood, I placed the two *Coco-Nuts* therein, laying them Side-ways, to prevent the *Moiſture* (which might deſcend from the Pots) from entring the Hole at the Baſe of the Fruit, and thereby rot the ſeminal Plant upon its firſt *germinating*. I then covered the Nuts over with the *Bark* two or three Inches thick, and placed the two Pots over them in their former Station. In this Place I let the Nuts remain for ſix Weeks; when removing the two Pots, and uncovering the Nuts, I found them both ſhot from the *Hole* in the *Baſe* of the Fruit, an Inch in Length; and from the other *End* of the Fruit were ſeveral Fibres emitted two or three Inches in Length. Upon finding them in ſuch a Forwardneſs, I took them out of the *Bark*, and planted them in large Pots, filled with *good freſh Earth*, plunging the Pots down to their Rims in the *Tanners-bark*, and covering the Surface of the Earth in the Pots half an Inch thick with the ſame: Soon after which the young Shoots were above two Inches long, and continued to thrive very well. I have communicated this Method ſince to ſome of my Acquaintance, who have tried it with the ſame Succeſs; and if the Nuts are freſh, ſcarce any of them miſcarry. This led me to try if the ſame Method would ſucceed as well with other *hard-ſhell'd, exotic Seeds*, which I could not, by any Method I had before tried, get to grow, as the *Bonduc*, or *Nicker-Tree*; the *Abrus*, or *Wild Liquorice*; the *Phaſeolus Braſilianus fruteſcens lobis villoſis*

loſis tungentibus maximus Hermanni, or *Horſe-eye Bean*; with ſeveral others; and I have found it both a ſure and expeditious Way to raiſe *any Sort* of *hard-ſhell'd Fruits*, or *Seeds*. For the *Heat* and *Moiſture* (which are abſolutely neceſſary to promote Vegetation) they here enjoy in an *equal* and *regular* Manner; the *Tanners-bark* (if rightly managed) keeping to near an *Equality* of Heat for ſix Months, and the *Water* which deſcends from the Pots, when they are watered, is by the Bark detained from being too ſoon diſſipated: which cannot be obtained in a common Hot-bed, the Earth in ſuch being worked away by the Water, and thereby leaving the Seeds often deſtitute of Moiſture. Some of theſe Seeds I have had ſhoot in a Fortnight's Time; which I am inform'd, would not have ſo done in a Month in their native Soil and Climate. I have alſo found this to be an excellent Method to reſtore Orange (or any other exotic) Trees, which have ſuffered by a tedious Paſſage, in being too long out of the Ground: Inſomuch I recovered two Orange-trees which had been ten Months without either Earth or Water. If this proves acceptable to you, it will be a Pleaſure to,

S I R,

Your moſt Obedient,

Humble Servant,

Philip Miller.

V. *An Account of the ſeveral Strata of Earths and Foſſils found in ſinking the mineral Wells at* Holt. *By the Reverend Mr.* Lewis, *Vicar of the Place. Communicated by* John Brome, *Eſq;*

AFter they had paſſed the upper Turf, they came to a blue Clay, which held about 3 Foot; then they met with a yellow, brittle Clay, very much like Ochre, uſed by Painters, about 2 Foot in Thickneſs; and next with a Loam of a looſer Texture, which ſparkled with a kind of Talc, called by the Naturaliſts Selenites, and was intermixed with yellow Ochre. Theſe Selenites, which were plentifully found ſhot in the Clay, were Chryſtals conſiſting of tranſparent, ſhining, brittle Flakes, ſome of a Rhomboidal, others of a Conical Figure, but all Hexacdra, or Columns of 6 Sides. They had no ſenſible Taſte of Salt, and the Clay in which they were found was interſperſed with Veins of coloured Earth, of the Colour of Sulphur and Iron Ruſt.

Below this, at about 10 Foot deep, they came to a Bed of Stones, of a large Size and very hard Texture, coated with Flakes of Gypſum of a white and yellowiſh Colour, which run through and divide them, as it were by various Membranes into different Cells, all filled with hardened Loam of a grey Colour. Theſe Stones, which were all of an oval Figure, in Shape reſembling Pebbles, weighed from 10 to 60 Pound Weight, and lay all on a Level one by another in the

Bed of Clay. Here the Springs come in, and below this the Clay was darker coloured, and interlaid with ſmall Shells of the Oyſter, Eſcallop and Muſcle Kind, and with a few Belemnites curiouſly ſhaped. Here they met with Stones of a very cloſe Texture, which when waſhed ſeemed to be nothing but a Maſs of Shells jumbled and embodyed together. And a little lower the Clay produced ſome Lumps of a black, bituminous Sulphur, interlaid with ſome ſmall thin Laminæ, ſeeming to be metalline and bright like the pureſt Silver: Upon firing this ſulphurous Bitumen on a red-hot Iron, it emitted a blue Flame, and ſtrong Smell like Brimſtone, but the Metal was loſt. From this Account of the different Strata found in ſinking theſe Wells, their Impregnation ſeems to be from Alum, Vitriol of Steel, Ochre and Sulphur, and from an accurate Mixture of all theſe, which no Art can imitate, it ſeems to derive thoſe admirable Qualities with which it is endued.

Some Conjecture may be made of its Nature and Qualities from the Tinctures it gives upon chymical Experiments: With aſtringent Drugs, as Galls, Oak-Leaves and Balauſtians, it ſometimes tinges Red, inclining to Purple, and ſometimes will not tinge at all: With volatile Alkalies, as Spirit of Urine, and Sal Armon. it turns milky, with lixiviate Salts, as Oil of Tartar, and Deliq. &c. it riſes in a white Curdle: But acid ſaline Liquors, as Spirit of Salt, Nitre, &c. cauſe no Alteration.

A Gallon and Half of this Water being evaporated *ad Siccitatem*, the Reliquiæ weighed 3 Drams, 1 Scruple and 19 Grains, ſome Parts of which were white, and ſhot into Stiriæ like Needles, and others into Priſms.

The

The neighbouring Country is chiefly a ſtrong Clay; the Quarries produce a very hard Stone, which ſeems to be a Compoſition of Shells cloſely cemented and embody'd together, and ſome Marchaſites which abound with Sulphur: In ſinking deep Pits they throw up Stones like Iron Oar, and covered with a ſhining metallick Subſtance and ſerpentine Stones, &c. and the plough'd Fields abound with Stones reſembling Shells of the Eſcallop and Cockle Kind, ſtriated with ſome Aſtroites, which are all ſtrong Alkalies, and with Aqua-fortis, or Spirit of Nitre, raiſe a violent Ebullition.

VI. *An Extract of a Letter of Signior* Michele Pinelli, *concerning the Cauſes of the Gout. Tranſlated from the* Italian *by* Joh. James Scheutzer, *M. D. F. R. S. and Coll. Med. Lond. Lic.*

IN Order to acquire a competent Knowledge of the various Diſtempers incident to the human Body, particularly ſuch, whoſe Cauſes being not very well known, their Cures alſo have been found hitherto very difficult, it is abſolutely neceſſary, that by the concurrent Aſſiſtance of Reaſon and Experience, we ſhould make an accurate Inquiry into the Principles of which it is compoſed.

With this general View, but more particularly in Order to diſcover the Nature and Cauſe of the Gout,

I have for ſome Time applied my ſelf to examin the ſeveral Parts of the human Body, whether ſoli or fluid, and by chymical Proceſſes to diſſolve them into their primitive or conſtituent Parts.

Thus, for Inſtance, I have found, that the ſoli Subſtance of the Bones, and the circumambient Parts, is compoſed of Phlegm, Oil, an alcaline volatile Salt, and a little Earth. The ſame Principles I have alſo conſtantly diſcovered in the Blood, and all the other Fluids, which are ſecreted from it, as well as the other ſolid Parts, with this Difference only, that there is more Phlegm, and leſs Oil and volatile Salt in the Fluids, than may be extracted from the Solids.

Carrying theſe Experiments ſtill further, and endeavouring to extract *viâ humidâ*, as the Chymiſts call it, the Salt from the Excrements in the Stomach and Inteſtins, I have conſtantly found a Salt not unlike Sea-ſalt, and appearing upon Examination to be a perfect Acid.

And yet, with all poſſible Art and Carefulneſs whatever, nothing of this acid Salt could ever be obtained from all the other ſolid, or fluid Parts of the Body.

Hence it appears how much thoſe muſt be miſtaken, who have hitherto aſſerted, that the Cauſe of the Gout is a coagulating Acid exiſting in the Blood, ſince nothing of an Acid is found in any Part of the Body, but the Excrements. This put me upon thinking, that the Cauſe of the Gout may be owing, perhaps, to that other ſaline Principle which chymical Trials ſhew us to exiſt both in the ſolid and fluid Parts:

And

And I have been confirmed herein by the following Experiments.

A very gouty Perſon being lately dead at *Rome*, I procured as much as I could of that tophaceous gouty Subſtance commonly found about the Joints of Perſons afflicted with this Diſtemper: And having got about three Ounces and a Half, I took ſix glaſs Bottles, and put ten Grains of it into each. The firſt of theſe Bottles I filled with diſtilled Vinegar, the ſecond with Spirit of Vitriol, the third with Spirit of Salt, the fourth with Spirit of Sal-armoniac, the fifth with Spirit of Hartſhorn, and the ſixth with Spirit of Urine. After four and twenty Hours, I found the aforeſaid tophaceous Matter totally diſſolved in the three firſt Bottles, which contained the acid Spirits, but in the three others, which I filled with alcaline Spirits, it remained entire and untouched, even for ſome Time after. From hence I concluded this tophaceous Matter to be of an alcaline Nature, foraſmuch as it is the Nature of acid Spirits, to diſſolve ſuch Subſtances as are either altogether alcaline, or compoſed in Part of an Alkali. And this alſo I conceived to be the Reaſon why the aforeſaid tophaceous Subſtance remained entire in the Bottles filled with alcaline Spirits, both being of the ſame Nature, and conſequently not to be diſſolved by each other.

But for a farther Satisfaction, I took the remaining Part of this tophaceous Matter, being about three Ounces, and put it into a ſmall Retort. Then having fixed a Recipient to it, I diſtilled it, according to the Rules of Art, by a gradual Fire, and obtained a Spirit, with ſome few Drops of Oil, about two Drams of a *Caput Mortuum* remaining in the Retort. This

Spirit

Spirit I found to be a perfect volatile Alcali, altogether of the ſame Nature with that which is extracted from Blood, from Urine, and from Bones. Hence again it is evident, that this tophaceous gouty Subſtance is compoſed of the ſame Principles with the other fluid and ſolid Parts of the human Body, or, that the Cauſe of the Gout is nothing elſe but a volatile, alcaline, corroſive Salt, which by corroding the ſenſible Membranes about the Joints, occaſions thoſe acute Pains, which we call the Gout.

FINIS.

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PHILOSOPHICAL TRANSACTIONS

FOR THE

Month of *OCTOBER*, 1728.

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Jos: Banks

I. *Of*

Fig. 1.

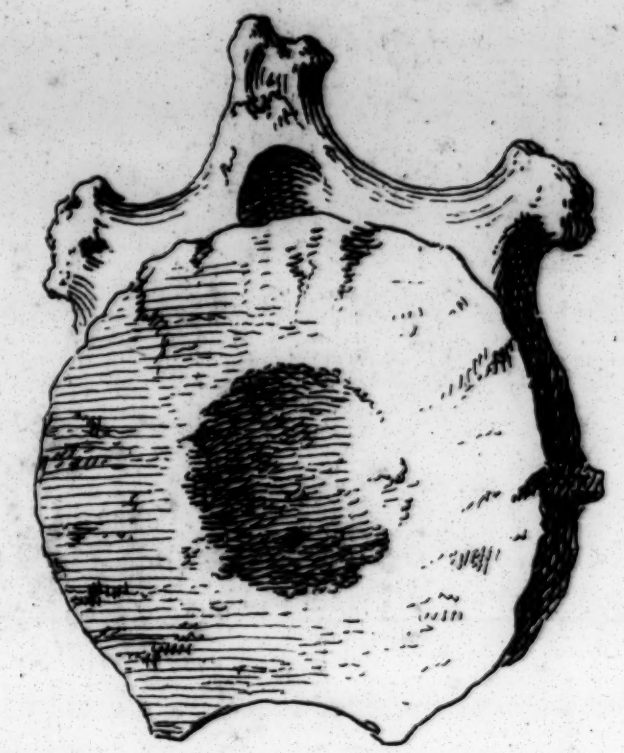


Fig. 3.

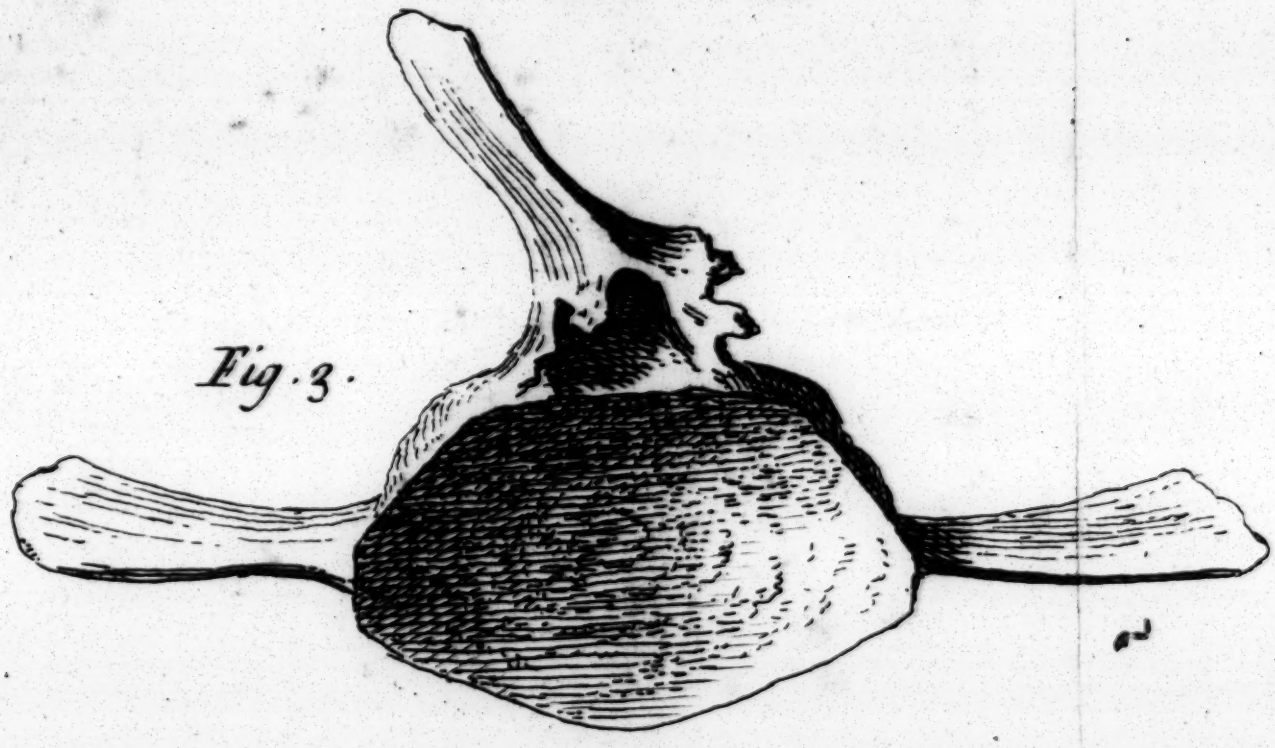
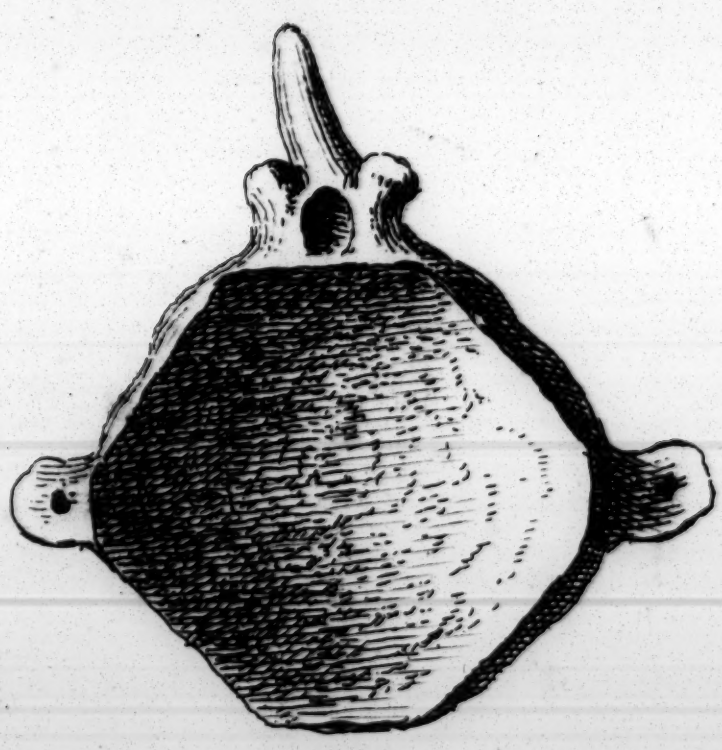
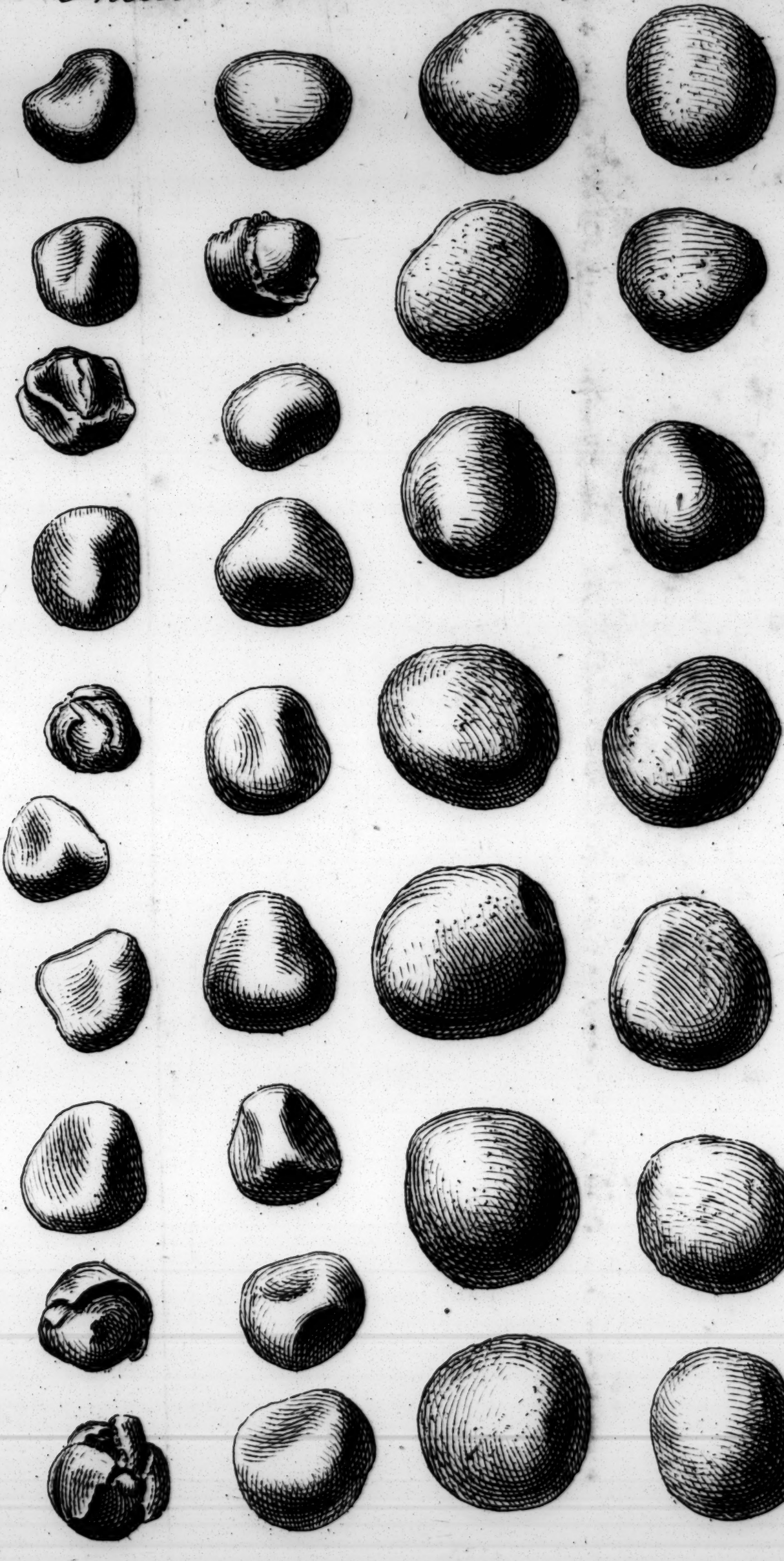


Fig. 2.





I. *Of* Foſſile Teeth *and* Bones *of* Elephants. *Part the Second.* *By Sir* Hans Sloane, *Bart.*

I Proceed now to the Second Part of this Diſcourſe, wherein I propoſe to offer ſome Remarks on divers Accounts of Bones and Teeth found under Ground, which I met with in ſeveral antient and modern Authors, and which will give me an Opportunity of examining into the Skeletons, and Parts of Skeletons, which are ſhewn up and down as undeniable Monuments of the Exiſtence of Giants.

And firſt, as many of thoſe Bones and Teeth, which are kept and ſhewn about for Bones and Teeth of Giants, have been found, upon a more accurate Inſpection, to be only the Bones and Teeth of Elephants or Whales, it may from thence very probably be inferr'd, that others alſo, which for want of a ſufficient Deſcription cannot be accurately enough accounted for, muſt have belonged either to theſe, or elſe ſome other large Animal. Thus the Fore-fin of a Whale, ſtripp'd of its Webb and Skin, was not long ago publickly ſhewn for the Bones of a Giant's Hand; and I have in my own Poſſeſſion (N° 1027) the Vertebra of the Loin of a large Whale, which was brought me from *Oxfordſhire*, where I was aſſured it was found under Ground, and afterwards made Uſe of for a Stool to ſit on (Fig. 1.). Now if a Computation had been made from the Proportion of this Vertebra to that of the other Parts of the Skeleton, and all had been ſuppoſed to

have belonged to a Man, ſuch a Skeleton would have exceeded in Meaſure, all thoſe fabulous Skeletons of Giants mentioned by Authors.

I cannot forbear on this Occaſion to obſerve, that it would be an Object well worthy the Inquiries of ingenious Anatomiſts, to make a Sort of comparative Anatomy of Bones; I mean to examine, with more Accuracy than hath been hitherto done, what Proportions the Skeletons and Parts of Skeletons of Men and Animals bear to each other, with Regard either to the Size, or Figure, or Structure, or any other Quality. This would doubtleſs lead us into many Diſcoveries, and is otherwiſe one of thoſe Things, which ſeem to be wanting to make Anatomy a Science ſtill more perfect and compleat. The very Vertebra I ſpeak of may ſerve to ſhew the Uſefulneſs of ſuch Obſervations. It differs in many Things from the Vertebræ of Men and Land-animals, as do the Vertebræ of Whales and the Fiſhes of the cetaceous Kind in general; and it is a very eaſy Matter to diſtinguiſh them from each other. The Body of the Vertebra is conſiderably larger in Proportion, and alſo lighter and more porous. The tranſverſe Proceſſes ariſe from the Middle of it on each Side. The oblique deſcending Proceſſes are altogether wanting; and the Arch, or Foramen, which the ſpinal Marrow paſſes through, is made up by the ſpinal Proceſs and the oblique aſcending ones only: The Body of the Vertebra is very rough and uneven on each End, full of ſmall Holes and Eminences, which receive the Holes and Eminences of a round Bone, or Plate, which anſwers to the Epiphyſis in a human Vertebra, whereof there are two between each Vertebra, joined together

together by an intermediate ſtrong and pretty thick Cartilage, probably to facilitate the Motion, and particularly the Flexion of theſe Animals in the Sea. (Fig. 2, 3.) But to return from this ſhort Digreſſion.

There are many Skeletons, that were from Time to Time found under Ground, and are mentioned by the Authors, who ſpeak of them, as Skeletons of Giants, and undeniable Monuments of their Exiſtence, which, as I have already obſerved, I ſhould rather take to be the Skeletons of Elephants, Whales, or ſome other huge Land or Sea-animal. Of this Kind ſeem to be the pretended Skeletons of Giants of 12, 20, and 30 Cubits in Height mentioned by *Philoſtratus* *: The Skeleton of ſix and forty Cubits in Height, which according to *Pliny* † was found in the Cavity of a Mountain in *Creta*, upon the overthrowing of that Mountain by an Earthquake: The Skeleton ſixty Cubits high, which *Strabo* ‡ ſays, was found near *Tingis* (now *Tangier*) in *Mauritania*, and was ſuppoſed to have been the Skeleton of *Anteus*: The Skeleton of *Pallas*, as pretended, found at *Rome* in the Year 1500, which was higher than the Walls of that City: And likewiſe that, which *Simon Majolus* ſays was found in *England* in the Year 1171: *Longè antè Fulgoſi ſæculum* (are his Words ||) *annis plus trecentis, anno ſcilicet* 1171. *in Anglia, illuvione fluminis, retecta ſunt humati olim Hominis oſſa adhuc ordine compoſita: Longitudo totius Corporis inventa eſt longa ad pedes quinquaginta.*

There are others, the Deſcription whereof concludes more clearly for their having once belonged to

* In ſuis Heroicis. † Hiſt. Nat. Lib. vii. C. xvi. ‡ Lib. xvii.
|| Dierum Canicularium Colloq. 11. *pag.* 36.

Elephants, though it could not be positively asserted, that they did. *S. Austin* *, discoursing of the Existence and great Feats of the Giants before the Deluge, mentions in Proof of what he advances, That he himself, with several others, saw at *Utica*, upon the Sea-shore, the Grinder of a Man so large, that if it had been cut into Teeth of an ordinary Size, at least an Hundred might have been made of it. *Hieronymus Magius* †, although himself very much prejudiced in Favour of the Existence of Giants, yet suspects this Tooth, mentioned by *S. Austin*, to have been rather the Tooth of an Elephant, or else some huge Creature of the Sea, than that of a Man. But *Ludqvicus Vives*, in his Commentaries upon that Passage of *S. Austin*, takes Notice, that in the Church of *S. Christopher* at *Hispella*, he was shewn a Tooth bigger than his Fist, which they pretended was one of the Teeth of that huge Saint, no Doubt, upon as good Ground, as that very large Shoulder-bone, which *Hieronymus Magius* says ‡, was shewn in a Church at *Venice*, was the Shoulder-bone of *S. Christopher*.

The pretended Skeleton of a Giant, which was found near *Drapani*, a Castle in *Sicily*, upon digging the Foundation of a House, and is described by *Joh. Boccatius* ||, is again not unlikely to have been the Skeleton of a large Elephant. For although the greatest Part of the Bones, through the Length of Time, and the Force of the subterraneal Steams, were so rotten, that after their being exposed to the Air, they fell to Pieces almost upon touching, yet three

* De Civit. Dei. Lib. xv. C. ix. citatus per Cassanionem & Lambecium. † Miscellaneorum Lib. i. C. ii. *pag.* 17. ‡ L. C. *pag.* 20. 6. || Genealogia degli Dei. L. iv. ad fin.

of

of the Teeth were found entire, which weighed an hundred Ounces, and were by the Inhabitants of *Drapani* hung up in one of their Churches, to perpetuate the Memory of this Fact. They likewiſe found Part of the Skull capacious enough to hold ſome Buſhels of Corn, and one of the Shank-bones, which was ſo large, that upon comparing it with the Shank-bone of an ordinary Man, it was judged, that this Giant, whom ſome took to be *Erick*, others *Ethellus*, others one of the *Cyclops*, and again others the renowned *Polyphemus* himſelf, muſt have been 200 Cubits high; according to which Calculation, he is figured and repreſented by *F. Kircher* * as by far the largeſt of a whole Gradation of Giants, whom, after this, he places in the following Order:

	Cubits.
The Giant of *Strabo*, whoſe Skeleton was dug up near *Tingis* in *Mauritania*, and was found to be — — — —	60 high.
Pliny's Giant, found in a Mountain in *Creta* — — — — —	46
The Skeleton of *Aſterius*, Son of *Anactes*	10
The Skeleton of *Oreſtes*, dug up by ſpecial Command of the Oracle — —	7
The Giant, whoſe Bones were found under a large Oak, not far from the Convent of *Reyden* in the Canton of *Lucern* in *Swiſſerland*.	9
Goliath, as deſcribed in Sacred Writs —	6 ½

The Caſe is ſtill leſs doubtful with Regard to thoſe Bones, which were found in *France* in 1456, in the

* Mund. Subterran. L. viii. Sect. 2.

Reign

Reign of *Charles* VII, by the Side of a River in the Barony of *Cruſſole* (afterwards erected into a County) not far from *Valence*. *Johannes Marius in Libris de Galliarum Illuſtrationibus*, *Calamæus in ſuis de Biturigibus Commentarijs*, *Fulgoſus* in *his Annals*, & *Joh. Caſſanio* of *Monſtroeuil*, in his Treatiſe of Giants *, ſeverally take Notice of theſe Bones, which were ſo large, that the whole Height of the Giant, to whom it was thought they belonged, and who was ſuppoſed to have been the Giant *Briatus*, was conjectured to have been of 15 Cubits. The Skull alone was two Cubits thick, and the Shoulder-bone ſix Cubits broad. Sometime after, other Bones of this Kind were found in the ſame Barony near the ſame Place, Part of which *Caſſanio* ſaw himſelf, and gives ſuch a particular Deſcription of one of the Teeth, as leaves little Room to doubt, but that it was the Grinder, and conſequently the other Bones, the Bones of an Elephant. His Words are †, *Miræ magnitudinis dentem multi ibidem conſpeximus, longitudine unius pedis, pondere librarum octo; multo autem oblongior quam craſſus viſus eſt, radiceſque aliquot habere quibus gingivæ inhærebat. Viſa eſt inſuper ea pars, qua cibus terebatur, aliquantulum concava, latitudine digitorum quatuor.* He adds farther, That ſuch another Tooth was kept at *Charmes*, a neighbouring Caſtle, that he meaſured the Length of the Place, whence theſe Bones were dug, and found it to be nine Paces; that ſome Time after more Bones were diſcovered at the ſame Place, and that the Country all thereabouts was very mountainous, and ſuch,

* *Pag.* 57, & ſeq. † *Pag.* 62.

as

as the Giants in all Probability delighted to dwell and command in. I have ſeen ſome of theſe Bones brought by a very curious *French* Merchant from this laſt mentioned Place, which I took to have belonged to an Elephant, by ſome large Cells between the Tables of the Skull, which are in the Skull of that Animal.

Hieronymus Magius * gives an Account of a very large Skull, eleven Spans in Circumference, and ſome other Bones, probably belonging to that Skull, which were dug up near *Tunis* in *Africa* by two *Spaniſh* Slaves, as they were ploughing in a Field. He was informed of this Matter by *Melchior Guilandinus*, who ſaw the Skull himſelf, when he had the Misfortune to be taken by the *Rovers*, and carried into Slavery to that Place in the Year 1559. I am the more inclined to believe, that this Skull and Bones was Part of the Skeleton of an Elephant, becauſe, as I ſhall ſhew hereafter, a like large Skeleton was dug up near the ſame Place ſome Time after, which by one of the Teeth ſent to *Peiresk* was made out to have been the Skeleton of an Elephant.

I now come to thoſe Bones, Teeth and Tusks, (or Horns, as ſome call them) which are mentioned by Authors to have been dug up in divers Parts of the World, and have been made out by them, or do otherwiſe appear by their Deſcription and Figures, indiſputably to belong to the Elephant.

Johannes Goropius Becanus †, notwithſtanding he lived in an Age, when the Stories of Giants were very much credited, and had found their Advocates, even

* Miſcellan. Lib. i. Cap. ii. *pag.* 19. 6. † Originum Antwerpianarum Libro. ii. quem Gigantomachiam appellavit, *pag.* 178.

among

among Perſons eminent for their Learning and Judgment, yet ventured to aſſert, that the Tooth, which was kept and ſhewn at *Antwerp*, as the Tooth of that unmerciful Giant, whoſe Defeat, brought about as they pretended, by *Brabo* a Son of *Julius Cæſar*, and King of the *Arcadians*, was fabulouſly reputed to have given Occaſion to the building of that Caſtle and City, was nothing but the Grinder of an Elephant. However diſpleaſing this Aſſertion might be, as *Goropius* farther adds, to thoſe who are delighted with ſuch idle and ridiculous Stories, yet to the Judicious it will appear the leſs ſurprizing, on Account of what paſſed not long before he wrote this Book, when the almoſt entire Skeletons of two Elephants, with the Grinders, and likewiſe the *dentes exerti*, or Tusks, were found near *Wielworda*, *Vilvorden*, as they were digging a Canal from *Bruſſels* to the River *Rupel*, to defend that Town and Country from the Incurſions of thoſe of *Mechlen*. *Goropius* conjectures, that theſe Elephants had been brought thither by the *Romans*, at the Time either of the Emperor *Galien*, or *Poſthumus*.

A very large Skeleton, likewiſe of a Giant, as pretended, was dug up near *Tunis* in *Africa* about the Year 1630, whereof one *Thomas d'Arcos*, who was then at that Place, ſent an Account, together with one of the Teeth, to the learned *Peiresk*. The Skull was ſo large, that it contained eight *Meilleroles* (a Meaſure of Wine in *Provence*) or one *Modius*, as *Gaſſendus* calls it *, or a Pint and a Half *Paris* Meaſure. Sometime after a live Elephant having been ſhewn at *Toulon*, *Peiresk* ordered, that he ſhould be brought to his Country Seat, on Purpoſe to take that Opportunity

* Gaſſendus in vita Peiresk. Lib. iv. Anno 1632.

to examine the Teeth of the Creature, the Impreſſions whereof he cauſed to be taken in Wax, and thereby found, that the pretended Giant's Tooth ſent him from *Tunis*, was only the Grinder of an Elephant. This is the ſecond large Skeleton dug up near *Tunis* in *Africa*, and it appearing plainly by the Tooth ſent to *Peiresk*, that it was the Skeleton of an Elephant, it may from thence very probably be conjectured, ſome other Circumſtances concurring, that the other alſo, which *Guilandinus* ſaw there, muſt have been rather of an Elephant, than of a Giant.

Thomas Bartholin * mentions the Grinder, or Maxillar-tooth of an Elephant, which was dug up in *Iſland*, and ſent to him by *Petrus Reſenius*. It was turned to a perfect ſtony Subſtance, like Flint, as was alſo the Tusk of a *Roſmarus*, dug up in the ſame Iſland.

A large Tooth, which by its Shape appears plainly to be the Grinder of an Elephant, is deſcribed and figured by *Lambecius* †, who had it out of the Emperor's Library, though he could not be informed where it was found, or how it got thither. It weighed 28 Ounces, and was commonly taken to be the Tooth of a Giant. *Antonius de Pozzis*, chief Phyſician to the Emperor, in a Letter to *Lambecius* ‡, affirms it to be an Elephant's Tooth, and conjectures, that it was dug up at *Baden*, about four Miles from *Vienna*, where, but a few Years before he wrote this Letter, they had found alſo the *Os Tibiæ & femoris* of an Elephant.

* Act. Medic. & Philoſoph. Hafn. To. I. Obſ. xlvi. *pag.* 83.
† Biblioth. Cæſar. Vindob. L. VI. *pag.* 311. ‡ Ib. Lib. vi. *pag.* 315.

Another Tooth, probably of an Elephant too, is deſcribed and figured by *Lambecius* *, who had it out of the Emperor's Library. It weighed 23 Ounces, and was found in the Year 1644 at *Krembs*, in the lower *Auſtria*, as they were increaſing the Fortifications of that Place.

The Year following, when the *Swedes* came to beſiege the Town of *Krembs*, a whole Skeleton of a Giant, as was pretended, was found at the Top of a neighbouring Mountain, near an old Tower. The Beſiegers, in their Intrenchments there, being very much incommoded by the Water that came down from the Mountains, dug a Ditch three or four Fathoms deep, to lead it another Way. It was in digging this Ditch they found the Skeleton aforeſaid, which was very much admired for its unuſual Size. Many of the Bones, chiefly thoſe of the Head, fell to Pieces upon being expoſed to the Air, others were broke by the Careleſſneſs of the Workmen; ſome eſcaped entire, and were ſent to learned Men in *Poland* and *Sweden*. Among theſe was a Shoulder-bone, with an Acetabulum in it large enough to hold a Cannon-ball. The Head, with Regard to its Bulk, was compared to a round Table, and the Bones of the Arms (or Fore-legs) as thick as a Man of an ordinary Size. One of the Grinders, weighing five Pounds, was given to the *Jeſuits* at *Krembs*: Another is figured by *Happelius* (in his *Relationes Curioſæ*, *Tom.* iv. *pag.* 47, 48.) to whom I am indebted for this Account, and it appears plainly by the Figure of it, that it is an Elephant's Tooth. It weighed four Pounds three Ounces *Nuremberg* Weight.

* Ib. Lib. vi. *pag.* 313.

Again,

Again, in *Lambecius* his *Bibliotheca Cæsarea Vindobonensis* *, are two Figures, and the Description of a very large Elephant's Tooth, which weighed 4 ¾ Pounds. It was sent from *Constantinople* to *Vienna* in 1678, and offered to be sold to the Emperor for 2000 *Rixdollars*, having been before, for its unusual Size, and pretended great Antiquity, valued at 10,000 *Rixdollars*. They pretended that it was found near *Jerusalem*, in a spatious subterranean Cavern, in the Grave of a Giant, which had the following Inscription upon it in the *Chaldaic* Language and Characters; *Here lies the Giant* OG; whence it was conjectured to have been the Tooth of *Og*, King of *Basan*, who was defeated by *Moses*, and who *only remained of the Remnants of Giants; whose Bed-stead was of Iron, nine Cubits was the Length thereof, and four Cubits the Breadth of it, after the Cubit of a Man* †. As the whole Story look'd very like an Imposition, the Emperor ordered, that the Tooth should be sent back again to *Constantinople*.

Hieronymus Ambrosius Langenmantel, a Member of the Imperial Academy of Sciences, inserted into the *Ephemerides* of that Academy ‡ an Abstract of a Letter to himself, from *Johannes Ciampini* in *Rome*, concerning some very large Bones, to wit the Shank-bone, the Shoulder-bone, and five Vertebræ, of the Number whereof was one of the Vertebræ of the Neck, which were dug up near *Vitorchiani*, in the Bishoprick of *Viterbo*, in the Year 1687. They weighed altogether upwards of 180 *Roman* Pounds, and having been compared with other the like Bones

* Lib. viii. *pag.* 652. † Deuteronom. Ch. iii. v. 2. ‡ Decur. ii. Annus vii. s. 1688. Obs. ccxxxiv. *pag.* 446.

in ſeveral Collections at *Rome*, particularly the *Chiſian* one, they appeared to be by far the largeſt. Moſt People took them to be the Bones of a Giant, but *Ciampini*, and ſome others, taking them, with more Probability, for the Bones of an Elephant, or ſome other large Animal, and knowing that there was in the Medicean Collection at *Florence* a compleat Skeleton of an Elephant, they procured a Copy of it, and found upon Compariſon, the above-mentioned Bones ſo exactly to correſpond with it, as to leave no Room to doubt, but that they had been Part of an Elephant's Skeleton.

The Skeleton of an Elephant which was dug up in a Sand-pit near *Tonna* in *Thuringen*, in 1695, is one of the moſt curious, and alſo the moſt compleat in its Kind, foraſmuch as they found the whole Head, with four Grinders, and the two *dentes exerti*, or Tusks, the Bones of the fore and Hind-legs, one of the Shoulder-bones, the Back-bones, with the Ribs, and ſeveral of the Vertebræ of the Neck. But the whole hath been ſo accurately deſcribed by *Wilhelmus Erneſtus Tentzelius*, Hiſtoriographer to the Dukes of *Saxony*, in a Letter to the learned *Magliabechi*, printed in the *Philoſophical Tranſactions* *, that it is needleſs to add any thing, the rather, as that Gentleman was pleaſed to oblige the *Royal Society* with ſome Pieces of the Bones of this Elephant, with Part of the Skull, wherein appeared its Cells, ſome of the Grinders, and Part of the *dentes exerti*; all which being produced at a Meeting of the *Royal Society*, were found exactly agreeable to his Deſcription, and ordered to be carefully preſerved in

* N° 234. *pag.* 737.

their Repoſitory. From the Surface of the Ground down to the Place where theſe Bones were found, the Diſpoſition of the *Strata*, or Layers, was as follows: A black Soil four Foot deep, Gravel two Foot and a Half, the Middle whereof was made up of Oſteocolla and Stones to the Depth of two Foot, Oſteocolla and Stones half a Foot, a ſandy Clay ſix Foot, with about two Inches of Oſteocolla in the Middle, Oſteocolla and Pebbles one Foot, Gravel ſix Foot, a white and fine Sand, the Depth whereof was unknown, and in this the Bones were found.

In the Second Volume of Count *Marſili*'s *Danubius*, where he treats of the Antiquities he obſerved along this River, there is Mention made of ſeveral Bones and Teeth of Elephants, which that inquiſitive Nobleman met with in *Hungary* and *Tranſylvania*, and which are now in his valuable Collection of natural and artificial Curioſities at *Bologna*. According to the beſt Information, the People of whom he had them could give him, they were found in Rivers, Lakes and Pools. One of the Vertebræ, a Grinder, and a conſiderable Part of the *dens exertus*, or Tusk, were found in the Lake, or Pool of *Hiulca*. Two Fragments of the *Os Tibiæ*, a little corroded on the Inſide, were taken out of a Pool near *Fogheras* in *Tranſylvania*, once the Seat of the Princes of that Country; and the whole lower Jaw, with two Grinders as yet ſticking in it, he had from ſome Fiſhermen, who found it in the ſtanding Waters by the River *Tibiſcus*, a little above *die Romerskantz*, or the *Roman Fort*. All theſe the Author cauſed to be figured as big as the Life. I have above related the Opinion of *Goropius* about the Antiquity of thoſe

two

two Elephants, the Skeletons whereof were found near *Vilvorden*, which he traces no higher than the Time of the *Romans*, and their Expeditions into thoſe Countries, particularly under *Galien* and *Poſthumus*. Count *Marſili* is of the ſame Opinion with Regard to thoſe Bones and Teeth found by him in *Tranſylvania*. He takes Notice, that whoſoever is acquainted with the vaſt Uſe the *Romans* made of Elephants in their military Expeditions, ought not to be ſurpriſed that there are Bones and Teeth found of them in thoſe Northern Countries, where otherwiſe there cannot have been any; and he urges, as a farther Proof of this Aſſertion, That they are found in Pools and Lakes, it having been the Cuſtom of the *Romans*, to throw the Carcaſſes of dead Elephants into the Water, as it is ſtill practiſed to this Day with the Carcaſſes of Horſes and other Beaſts, to prevent the Diſtempers and other Inconveniencies, which their Putrefaction might otherwiſe occaſion. On the other Hand, there are many Arguments, taken from the Largeneſs of the Beaſts, the Skeletons whereof are thus found under Ground, which ſometimes far exceeds any that was, or could have been brought alive into *Europe*, from the Condition they are found in, and from the particular Diſpoſition of the Strata above the Places where they are found, whereby it appears, almoſt to a Demonſtration, that they muſt be of much greater Antiquity, and that they cannot have been buried at the Places where they are found, or brought thither any otherwiſe, but by the Force of the Waters of an univerſal Deluge. To inſiſt only upon one of theſe Arguments: If the Skeletons of Elephants, which are thus found under Ground,

Ground, and at conſiderable Depths too, had been buried there either by the *Romans*, or any other Nation, the Strata above them muſt neceſſarily have been broke through and altered; whereas on the contrary ſeveral Obſervations inform us, that they were found entire, whence it evidently appears, that what is found underneath, muſt have been lodged there, if not before, at leaſt at the very Time when theſe Strata were formed; conſequently long before the *Romans*. But there is another Argument, which ſeems to me to bear very hard againſt the Conjectures of *Goropius* and Count *Marſili*. *Tentzelius* hath already mentioned it, and it is urged from the great Value of Ivory at all Times, and particularly among the *Romans*, which appears by many Paſſages in antient Authors; as for Inſtance, by a very remarkable one in *Pliny* *, who takes Notice, That among the valuable Preſents, which the *Ethiopians* were obliged to make to the Kings of *Perſia*, by Way of a Tribute, there were twenty large Teeth (unqueſtionably the *dentes exerti*) of Elephants, and then adds, *Tanta ebori auctoritas erat*. Now it is to be preſumed, that the *Romans* would not have neglected to take away the Teeth, and particularly the *dentes exerti* of dead Elephants, before they flung their Carcaſſes into the Water, whereas there hath ſcarce been any Skeleton, or any Part of the Skeleton of an Elephant dug up any where, but the Teeth were found along with them, and even among thoſe figured by Count *Marſili*, there are three Grinders, and a conſiderable Part of one of the *dentes exerti*.

* Lib. xii. C. 4.

Dr. *Robert Plot* in his *Natural Hiſtory of* Staffordſhire *, ſays, That he was preſented by *William Leveſon Gower* of *Trentham*, Eſq; with the lower Jaw of ſome Animal, with large Teeth ſticking in it, dug up in a Marle-pit in his Ground, and which upon Compariſon he found exactly agreeable to the lower Jaw of the Elephant's Skull in Mr. *Aſhmole*'s *Muſeum* at *Oxford*.

In the *Muſeum* of the *Royal Society* there are two Foſſil-bones of Elephants: One was given by Sir *Thomas Brown* of *Norwich*, the other was brought from *Syria* for the *Os Tibiæ* of a Giant, but Dr. *Grew* † proves by an exact Computation, that it can never have been the *Os Tibiæ* of a human Skeleton, by being full 20 times as thick, and but three times as long. It is an *Engliſh* Yard and half a Foot long, and hath a Foot in Circumference, where it is thinneſt. Dr. *Grew* obſerves, that by the Figure it appears to have belonged to the Leg, and not to the Thigh, and he conjectures the whole Elephant to have been about five Yards high.

Before I diſmiſs this Subject, I muſt beg Leave to mention a few more. *Geſſner* ‡ takes Notice, that he was preſented by a *Poliſh* Nobleman with a Tooth four times as large as that, which he figured under the Title of *Hippopotamus* in his Book *de Aquatilibus*. It was found under Ground, as they were digging for the Foundation of a Houſe, together with a very large Horn, as they called it, which many took to be an Unicorn's Horn, but wrongly, as he, *Geſſner*, thought, becauſe of its being too thick and

* Ch. vii. §. 78. *pag.* 78. † Muſæum. Reg. Soc. *pag.* 32. ‡ De Figuris Lapidum. *pag.* 157.

too

too crooked. It is very probable, that this pretended Horn was the *dens exertus* of an Elephant. The ſame Author mentions a ſubterraneous Cavern near *Elbingeroda*, wherein were found the Bones and Teeth of Men and Animals ſo large, that it was ſcarce credible, that ever any of that bulky Size ſhould have exiſted.

The Grinder of an Elephant, petrified, is kept in the King of *Denmark* his Cabinet at *Copenhagen*, as appears by the Catalogue *, but there is no Mention made how it came thither, or where it was found.

They ſhew in the ſame Collection a large Thigh-bone, which weighs about twenty *Daniſh* Pounds, and is above three Foot in Length †. It is ſo old, according to the Author of the Catalogue, that it is almoſt become ſtony. The ſame Author takes Notice of another large Bone, then in the Collection of *Otho Sperling*, which weighed 25 Pounds, and was four Foot long. It was, as *Sperling* told him, found in the Year 1643 at *Bruges* in *Flanders*, near the publick Priſon, in Preſence of *Bernard de Arauda*, and his, *Sperling*'s, Father, who ſaw the whole Skeleton there, which was of 20 Yards of *Brabant* in Length.

A Piece of Ivory was dug up in a Field on the River *Viſtula*, about ſix Miles from *Warſaw*, which having been ſhewn at *Dantzick* to *Gabriel Rzaczynski*, Author of the *Natural Hiſtory of* Poland, it ſeemed to him to be the *dens exertus* of an Elephant ‡.

* Muſ. Regium. Part I. Sect. vii. N° 109. † Ibid. Part I. Sect. i. N° 73. ‡ Rzaczynski Hiſt. Nat. Reg. Polon. *pag.* 2.

In the Notes upon the laſt Edition of Dr. *Herman's Cynoſura Medica*, publiſhed by Dr. *Boecler* of *Strasburg* *, under the Title of *Unicornu Foſſile*, there is Mention made of a remarkable Piece of Foſſil Ivory, or rather of an Elephant's Tooth, in the Hands of *Jaques Samſon de Rathſamhauſen de Ehenweyer*, an *Alſatian* Nobleman. It was found in the *Rhine* upon one of his Eſtates near *Nonneville*, and was three *Paris* Foot, three Inches and a Half long: It had near a Foot at the Baſis in Circumference, where thickeſt, and about 8 Inches and a Half at the other Extremity. It was filled within with a Sort of Marl, but the outward Surface was ſtony in ſome Places, and bony in others. The bony Part ſcrap'd, or burnt, ſmell'd like Ivory. The Scrapings boil'd made a Sort of Gelly. The Author of the Notes adds, That they find Foſſil Ivory in ſeveral Parts of *Europe*, particularly in the *Schwartzwald* (*Sylva Hercynia*) in *Moravia*, in *Saxony*, and near *Canſtad* in the Dutchy of *Wirtemberg*.

* 1726. 4*to*. P. iii. *pag*. 133.

II. *A Me-*

II. *A Method for determining the Number of impossible Roots in adfected Æquations.* By Mr. George Campbell.

Lemma I.

IN every adfected quadratick Æquation $ax^2 - Bx + A = 0$, whofe Roots are real, a fourth Part of the Square of the Coefficient of the fecond Term is greater than the Rectangle under the Coefficient of the firft Term and the abfolute Number or $\frac{1}{4}B^2 \searrow a \times A$; and *vice verfa* if $\frac{1}{4}B^2 \searrow a \times A$, the Roots of the Æquation $ax^2 - Bx + A = 0$, will be real. But if $\frac{1}{4}B^2 \nsearrow a \times A$, the Roots will be impoffible. This is evident from the Roots of the Æquation being $\frac{\frac{1}{2}B + \sqrt{\frac{1}{4}B^2 - a \times A}}{a}$, $\frac{\frac{1}{2}B - \sqrt{\frac{1}{4}B^2 - a \times A}}{a}$.

Lemma II.

Whatever be the Number of impoffible Roots in the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + \&c. \pm dx^3 \mp cx^2 \pm bx \mp A = 0$, there are juft as many in the Æquation $Ax^n - bx^{n-1} + cx^{n-2} - dx^{n-3} + \&c. \pm Dx^3 \mp Cx^2 \pm Bx \mp 1 = 0$. For the Roots of the laft Æquation are the Reciprocals of thofe of the firft, as is evident from common Algebra. Let the Roots of the biquadratick Æquation $x^4 - Bx^3 + Cx^2 - Dx + A = 0$ be a, b, c, d, whereof let c, d be impoffible, then the Roots of the Æquation

$Ax^4 - Dx^3 + Cx^2 - Bx + 1 = 0$ will be $\frac{1}{a}, \frac{1}{b}, \frac{1}{c}, \frac{1}{d}$, and therefore two of them to wit $\frac{1}{c}, \frac{1}{d}$ impoſſible.

Lemma III.

In every Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + Ex^{n-4} - \&c. \pm ex^4 \mp dx^3 \pm cx^2 \mp bx \pm A = 0$, all whoſe Roots are real, if each Term be multiply'd by the Index of x in that Term, and each Product be divided by x, the reſulting Æquation $nx^{n-1} - \overline{n-1}Bx^{n-2} + \overline{n-2}Cx^{n-3} - \overline{n-3}Dx^{n-4} + \overline{n-4}Ex^{n-5} - \&c. \pm 4ex^3 \mp 3dx^2 \pm 2cx \mp b = 0$ ſhall have all its Roots real. Thus if all the Roots of the Æquation $x^4 - Bx^3 + Cx^2 - Dx + A = 0$ be real, then all the Roots of the Æquation $4x^3 - 3Bx^2 + 2Cx - D = 0$ will alſo be real. This Lemma doth not hold converſly, for there are an Infinity of Caſes where all the Roots of the Æquation $nx^{n-1} - \overline{n-1}Bx^{n-2} + \overline{n-2}Cx^{n-3} - \overline{n-3}Dx^{n-4} + \&c. \pm 3dx^2 \mp 2cx \pm b = 0$ are real, at the ſame Time ſome or perhaps all the Roots of the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + \&c. \pm dx^3 \mp cx^2 \pm bx \mp A = 0$ are impoſſible: But whatever be the Number of impoſſible Roots in the Æquation $nx^{n-1} - \overline{n-1}Bx^{n-2} + \overline{n-2}Cx^{n-3} - \&c. \pm 2cx \mp b = 0$, there are at leaſt as many in the Æquation $x^n - Bx^{n-1} + Cx^{n-2} \&c. \pm cx^2 \mp bx \pm A = 0$. Thus all the Roots of the Æquation $4x^3 - 3Bx^2 + 2Cx - D = 0$ may be real, and yet two or perhaps all the four

four Roots of the Æquation $x^4 - Bx^3 + Cx^2 - Dx + A = 0$ may be impoſſible, but if two of the Roots of the Æquation $4x^3 - 3Bx^2 + 2Cx - D = 0$ be impoſſible, there muſt be at leaſt two impoſſible Roots in the Æquation $x^4 - Bx^3 + Cx^2 - Dx + A = 0$. All this hath been demonſtrated by Algebraical Writers, particularly by Mr. *Reyneau* in his *Analyſe Demontré*, and is eaſily made evident by the Method of the *Maxima* and *Minima*.

COROLARY. Let all the Roots of the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + Ex^{n-4} - Fx^{n-5} + \&c. \pm fx^5 \mp ex^4 \pm dx^3 \mp cx^2 \pm bx \mp A = 0$ be real, and by this Lemma all the Roots of the Æquation $nx^{n-1} - \overline{n-1}Bx^{n-2} + \overline{n-2}Cx^{n-3} - \overline{n-3}Dx^{n-4} + \overline{n-4}Ex^{n-5} - \overline{n-5}Fx^{n-6} + \&c. \pm 5fx^4 \mp 4ex^3 \pm 3dx^2 \mp 2cx \pm b = 0$ will be real, and therefore (by the ſame Lemma) all the Roots of the Æquation $n \times \overline{n-1}x^{n-2} - \overline{n-1} \times \overline{n-2}Bx^{n-3} + \overline{n-2} \times \overline{n-3}Cx^{n-4} - \overline{n-3} \times \overline{n-4}Dx^{n-5} + \overline{n-4} \times \overline{n-5}Ex^{n-6} - \overline{n-5} \times \overline{n-6}Fx^{n-7} + \&c. \pm 20fx^3 \mp 12ex^2 \pm 6dx \mp 2c = 0$

or (dividing all by 2) of $n \times \frac{\overline{n-1}}{2}x^{n-2} - \overline{n-1} \times \frac{\overline{n-2}}{2}Bx^{n-3} + \overline{n-2} \times \frac{\overline{n-3}}{2}Cx^{n-4} - \&c. \pm 10fx^3 \mp 6ex^2 \pm 3dx \mp c = 0$ will be real. After the ſame Manner all the Roots of the Æquation

$$n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3}x^{n-3} - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \frac{\overline{n-3}}{3}Bx^{n-4} +$$

$B x^{n-1} + \overline{n-2} \times \frac{\overline{n-3}}{2} \times \frac{\overline{n-4}}{3} C x^{n-3} - \&c. \pm$ to $fx^2 \mp 4ex \pm d = 0$ will be real; and thus we may descend until we arrive at the quadratick Æquation $n \times \frac{\overline{n-1}}{2} x^2 - \overline{n-1} B x + C = 0$. The same Æquations do ascend thus $n \times \frac{\overline{n-1}}{2} x^2 - \overline{n-1} B x + C = 0$, $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} x^3 - \overline{n-1} \times \frac{\overline{n-2}}{2} B x^2 + \overline{n-2} C x - D = 0$, $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times \frac{\overline{n-3}}{4} x^4 - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \frac{\overline{n-3}}{3} B x^3 + \overline{n-2} \times \frac{\overline{n-3}}{2} \times C x^2 - \overline{n-3} D x + E = 0$, $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times \frac{\overline{n-3}}{4} \times \frac{\overline{n-4}}{5} x^5 - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \frac{\overline{n-3}}{3} \times \frac{\overline{n-4}}{4} B x^4 + \overline{n-2} \times \frac{\overline{n-3}}{2} \times \frac{\overline{n-4}}{3} C x^3 - \overline{n-3} \times \frac{\overline{n-4}}{2} D x^2 + \overline{n-4} E x - F = 0$, and so on. Let M represent any of the Coefficients of the Æquation $x^n - B x^{n-1} + C x^{n-2} - D x^{n-3} + E x^{n-4} - \&c. \pm A = 0$, and let L N be the adjacent Coefficients, let M be the Exponent of the Coefficient M: By the Exponent of a Coefficient I mean the Number which

expresseth

expreſſeth the Place which it hath among the Coefficients, thus if M repreſent the Coefficient E (and therefore $L = D$ and $N = F$) then $m = 4$. It will be eaſy to ſee, that, amongſt the foregoing aſcending Æquations, that which hath its abſolute Number N will be $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times$ &c. $\frac{\overline{n-m}}{m+1} x^{m+1} - \overline{n-1} \times \frac{\overline{n-2}}{2} \times$ &c. $\frac{\overline{n-m}}{m} B x^m +$ $\overline{n-2} \times$ &c. $\frac{\overline{n-m}}{m-1} C x^{m-1} -$ &c. $\pm \overline{n-m+1} \times$ $\frac{\overline{n-m}}{2} L x^2 \mp \overline{n-m} M x \pm N = 0$, all whoſe Roots are real when all the Roots of the Æquation $x^n - B x^{n-1} + C x^{n-2} -$ &c. $\pm A = 0$ are real. Let $N = F$ and therefore $M = E$, $L = D$ and $m = 4$, then that of the aſcending Æquations whoſe abſolute Number is F, will be $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times$ $\frac{\overline{n-3}}{4} \times \frac{\overline{n-4}}{5} x^5 - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \frac{\overline{n-3}}{3} \times \frac{\overline{n-4}}{4}$ $B x^4 + \overline{n-2} \times \frac{\overline{n-3}}{2} \times \frac{\overline{n-4}}{3} C x^3 - \overline{n-3} \times \frac{\overline{n-4}}{2}$ $D x^2 + \overline{n-4} E x - F = 0$.

PRO-

PROPOSITION I.

Let $x^n - Bx^{n-1} + Cx^{n-2} - \mathcal{D}x^{n-3} + Ex^{n-4} - \&c. \pm ex^4 \mp dx^3 \pm cx^2 \mp bx \pm A = 0$ be an Æquation of any Dimenſions all whoſe Roots are real, let M be any Coefficient of this Æquation, L, N the adjacent Coefficients, and m the Exponent of M. Then the Square of any Coefficient M multiply'd by the Fraction $\frac{\overline{m \times n-m}}{\overline{m+1} \times \overline{n-m+1}}$ will always exceed the Rectangle under the adjacent Coefficients $L \times N$. Thus in the Æquation $x^4 - Bx^3 + Cx^2 - \mathcal{D}x + A = 0$, where $n = 4$, making $M = C$ and therefore $L = B$, $N = \mathcal{D}$, and $m = 2$, then $\frac{\overline{2 \times 4-2}}{\overline{2+1} \times \overline{4-2+1}} \times C^2$ or $\frac{4}{9}C^2$ will exceed $B \times \mathcal{D}$ providing all the Roots of the Æquation be real.

Becauſe (by Lem. 3.) the Roots of the quadratick Æquation $n \times \frac{\overline{n-1}}{2}x^2 - \overline{n-1}Bx + C = 0$, are real, therefore (by Lem. 1.) $\frac{1}{4}\overline{n-1}|^2 \times B^2$ muſt be greater than $n \times \frac{\overline{n-1}}{2} \times C$ and (dividing both by $n \times \frac{\overline{n-1}}{2}$) $\frac{\overline{n-1}}{2n} \times B^2$ greater than $1 \times C$. Therefore in the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - \mathcal{D}x^{n-3} + \&c. \pm A = 0$ of the n Degree, all whoſe Roots are real, the Square of B the Coefficient of the ſecond Term,

Term, multiply'd by the Fraction $\frac{n-1}{2n}$ is greater than $1 \times C$ the Rectangle under the adjacent Coefficients. But (by Lem. 2.) all the Roots of the Æquation $Ax^n - bx^{n-1} + cx^{n-2} - \&c. \pm Cx^2 \mp Bx \pm 1 = 0$ or (dividing by A) of $x^n - \frac{b}{A}x^{n-1} + \frac{c}{A}x^{n-2} - \&c. \pm \frac{C}{A}x^2 \mp \frac{B}{A}x \pm \frac{1}{A} = 0$ are real, therefore (from what hath been juſt now ſaid) $\frac{n-1}{2n} \times \frac{b^2}{A^2}$ muſt be greater than $1 \times \frac{c}{A}$ and conſequently $\frac{n-1}{2n} \times b^2$ greater than $c \times A$. Therefore in an Æquation $x^n - Bx^{n-1} + Cx^{n-2} - \&c. \pm cx^2 \mp bx \pm A = 0$, of the n Degree, all whoſe Roots are real, the Square of the Coefficient of x multiply'd by the Fraction $\frac{n-1}{2n}$ is greater than the Rectangle under the Coefficient of x^2 and the abſolute Number. But by Cor. Lem. 3. all the Roots of the Æquation $n \times \frac{n-1}{2} \times \frac{n-2}{3} \times \&c. \times \frac{n-m}{m+1}x^{m+1} - \frac{n-1}{2} \times \frac{n-2}{3} \times \&c. \times \frac{n-m}{m}Bx^m + \frac{n-2}{2} \times \&c. \frac{n-m}{m-1}Cx^{m-1} \&c. \pm \frac{n-m+1}{2} \times Lx^2 \mp$

$\overline{n-m}\, M x \pm N = o$ are real, therefore (ſeeing this Æquation is of the $m+1$ Degree) the Square of $\overline{n-m} \times M$ multiply'd by the Fraction $\frac{\overline{m+1-1}}{2 \times m+1}$ will be greater than the Rectangle under $\overline{n-m+1} \times \frac{\overline{n-m}}{2} \times L$ and N, that is $\frac{m}{2 \times m+1} \times \overline{n-m}^{2} \times M^2$ will be greater than $\overline{n-m+1} \times \frac{\overline{n-m}}{2} \times L \times N$ and therefore (dividing both by $\overline{n-m+1} \times \frac{\overline{n-m}}{2}$) $\frac{m \times \overline{n-m}}{m+1 \times \overline{n-m+1}} \times M^2$ greater than $L \times N$.

COROLARY. Make a Series of Fractions $\frac{n}{1}, \frac{n-1}{2}, \frac{n-2}{3}, \frac{n-3}{4}$, &c. unto $\frac{1}{n}$ whoſe Denominators are Numbers going on in the Progreſſion 1, 2, 3, 4, &c. unto the Number n which is the Dimenſions of the Æquation $x^n - B x^{n-1} + C x^{n-2} -$ &c. $\pm A = o$, and whoſe Numerators are the ſame Progreſſion inverted. Divide the ſecond of theſe Fractions by the firſt, the third by the ſecond, the fourth by the third, and ſo on, and place the Fractions which reſult from this Diviſion above the middle Terms of the Æquation, thus $x^n - \frac{n-1}{2n} B x^{n-1} + \frac{2 \times \overline{n-2}}{3 \times n-1} C x^{n-2} - D x^{n-3} +$

$\frac{3 \times n-3}{4 \times n-2} \mathcal{D} x^{n-3} + \frac{4 \times n-4}{5 \times n-3} E x^{n-4} - \&c. \pm A = 0$. Then if all the Roots of the Æquation are real, the Square of any Coefficient multiply'd by the Fraction which ſtands above, will be greater than the Rectangle under the adjacent Coefficients. This Corolary doth not hold converſly, for there are an Infinity of Æquations in which the Square of each Coefficient multiply'd by the Fraction above it, may be greater than the Rectangle under the adjacent Coefficients, and notwithſtanding ſome or perhaps all of the Roots may be impoſſible. Therefore when the Square of a Coefficient multiply'd by the Fraction above, is greater than the Rectangle under the adjacent Coefficients, from this Circumſtance nothing can be determined as to the Poſſibility or Impoſſibility of the Roots of the Æquation: But when the Square of a Coefficient multiply'd by the Fraction above it, is leſs than the Rectangle under the adjacent Coefficients, it is a certain Indication of two impoſſible Roots. From what hath been ſaid, is immediately deduced the Demonſtration of that Rule which the moſt illuſtrious *Newton* gives for determining the Number of impoſſible Roots in any given Æquation.

SCHOLIUM.

Let the Roots of the Æquation $x^n - B x^{n-1} + C x^{n-2} - \mathcal{D} x^{n-3} + E x^{n-4} - F x^{n-5} + \&c. \pm A = 0$ (with their Signs) be repreſented by the Letters $a, b, c, d, e, f, g, \&c.$ then (as is commonly known) B will be the Sum of all the Roots or $= a + b + c + d + e + f + \&c.$ C the Sum of the Products

of all the Pairs of Roots or $= ab + ac + ad + af + ag +$ &c. D the Sum of the Products of all the Ternaryes of Roots or $= abc + abd + abe + abf + abg +$ &c. $E = abcd + abce + abcf + abeg +$ &c. $F = abcde + abcdf + abcdg + bcdef +$ &c. and ſo on. Let (as in this Propoſition) M repreſent any of theſe Coefficients, L, N the adjacent Coefficients, and m the Exponent of M; let Z repreſent the Sum of the Squares of all the poſſible Differences between the Terms of the Coefficient M, let α be the Sum of all thoſe of the foreſaid Squares whoſe Terms differ by one Letter, β the Sum of all thoſe Squares whoſe Terms differ by two Letters, γ the Sum of thoſe Squares whoſe Terms differ by three Letters, δ the Sum of thoſe Squares whoſe Terms differ by four Letters and ſo on. Thus if $M = F = abcde + abcdf + abcdg +$ &c. then $Z = \overline{abcde - abcdf}|^2 + \overline{abcde - abcdg}|^2 + \overline{abcde - abcfg}|^2 + \overline{bcdef - abfgh}|^2 +$ &c. $\alpha = \overline{abcde - abcdf}|^2 + \overline{abcde - abcdg}|^2 + \overline{abcde - abcdh}|^2 + \overline{bcdef - bcdeg}|^2 +$ &c. $\beta = \overline{abcde - abcfg}|^2 + \overline{abcde - abcfh}|^2 + \overline{bcdef - acdfh}|^2 +$ &c. $\gamma = \overline{abcde - abfgh}|^2 + \overline{abcdf - abegh}|^2 +$ &c. $\delta = \overline{abcde - afghk}|^2 + \overline{acdfg - abehk}|^2 +$ &c. This being laid down I ſay that the Square of any Coefficient M multiply'd by the Fraction $\frac{m \times \overline{n - m}}{\overline{m + 1} \times \overline{n - m + 1}}$ exceeds the Rectangle under the adjacent Coefficients $L \times N$ by $\overline{n + 1} \times Z$

$\frac{\overline{n+1} \times Z}{\overline{m+1} \times \overline{n-m+1}} - \frac{1}{2}\alpha - \frac{1}{3}\beta - \frac{1}{4}\gamma - \frac{1}{5}$

$\delta -$ &c. The Series $- \frac{1}{2}\alpha - \frac{1}{3}\beta - \frac{1}{4}\gamma -$ &c. muſt conſiſt of m Number of Terms.

Let the Æquation be $x^5 - B x^4 + C x^3 - D x^2 + E x - A = 0$, whoſe Roots let be a, b, c, d, e, in which Caſe $n = 5$. Let $M = B = a + b + c + d + e$, then $L = 1$, $N = G$, $m = 1$, $Z = \overline{a-b}|^2 + \overline{a-c}|^2 + \overline{a-d}|^2 + \overline{a-e}|^2 + \overline{b-c}|^2 +$ &c. $= \alpha$; therefore $\frac{1 \times 5 - 1}{1+1 \times 5 - 1 + 1} \times$

B^2 or $\frac{2}{5} B^2$ exceeds $1 \times C$ by $\frac{\overline{5+1} \times Z}{\overline{1+1} \times \overline{5-1+1}}$

$- \frac{1}{2}\alpha = \frac{3}{5} Z - \frac{1}{2}\alpha =$ (becauſe $Z = \alpha$)

$\frac{1}{10} Z = \frac{1}{10}\overline{a-b}|^2 + \frac{1}{10} \times \overline{a-c}|^2 + \frac{1}{10}\overline{a-d}|^2 +$ &c. which is always a poſitive Number when the Roots a, b, c, d, e are real, poſitive or negative Numbers. Let $M = C = a\,b + a\,c + a\,d + a\,e + b\,c +$ &c. then $L = B$, $N = D$, $m = 2$, $Z = \overline{a\,b - a\,c}|^2 + \overline{a\,b - a\,d}|^2 + \overline{a\,b - c\,d}|^2 + \overline{a\,b - d\,e}|^2 +$ &c. $\alpha = \overline{a\,b - a\,c}|^2 + \overline{a\,b - a\,d}|^2 + \overline{a\,b - a\,e}|^2 +$ &c. $\beta = \overline{a\,b - c\,d}|^2 + \overline{a\,b - c\,e}|^2 + \overline{a\,b - d\,e}|^2 +$ &c. therefore $\frac{2 \times 5 - 2}{2+1 \times 5 - 2 + 1} \times C^2$

or

or $\frac{1}{2} C^2$ furpaſſeth $B \times D$ by $\frac{\overline{5+1} \times Z}{\overline{2+1} \times \overline{5-2+1}}$ $- \frac{1}{2}\alpha - \frac{1}{3}\beta =$ (becauſe $Z = \alpha + \beta$) $= \frac{1}{6}$ $\beta = \frac{1}{6} \times \overline{ab-cd}|^2 + \frac{1}{6}\overline{ab-ce}|^2 + \frac{1}{6} \times$ $\overline{ab-de}|^2 +$ &c. which is always a poſitive Number when the Roots a, b, c, d, e are real Numbers, poſitive or negative. Let $M = D = abc + abd + abe + acd + ace +$ &c. then $L = C$, $N = E$, $m = 3$, $Z = \overline{abc-abd}|^2 + \overline{abc-abe}|^2 + \overline{abc-ade}|^2 +$ &c. $\alpha = \overline{abc-abd}|^2 + \overline{abc-abe}|^2 + \overline{abc-acd}|^2 +$ &c. $\beta = \overline{abc-ade}|^2 + \overline{abc-cde}|^2 + \overline{abc-bde}|^2 +$ &c. $\gamma = 0$, therefore $\frac{\overline{3 \times 5-3}}{\overline{3+1} \times \overline{5-3+1}} \times$ D^2 or $\frac{1}{2} D^2$ exceeds $C \times E$ by $\frac{\overline{5+1}}{\overline{3+1} \times \overline{5-3+1}} \times$ $Z - \frac{1}{2}\alpha - \frac{1}{3}\beta =$ (becauſe $Z = \alpha + \beta$) $= \frac{1}{6} \times$ $\beta = \frac{1}{6} \times \overline{abc-ade}|^2 + \frac{1}{6} \times \overline{abc-cde}|^2 + \frac{1}{6}$ $\overline{abc-bde}|^2 +$ &c. which is a poſitive Number when the Roots are real Numbers. Let $M = E =$ $abcd + abce + abde + bcde +$ &c. then $L = D$, $N = A$, $m = 4$, $Z = \overline{abcd-abce}|^2 +$ $\overline{abcd-bcde}|^2 + \overline{abcd-acde}|^2 +$ &c. $= \alpha$, $\beta =$

$\beta = 0 = \gamma = \delta$, therefore $\frac{\overline{4 \times 5 - 4}}{4+1 \times 5 - 4+1} \times E^2$ or $\frac{2}{5} E^2$ exceeds $\mathcal{D} \times A$ by $\frac{\overline{5+1}}{4+1 \times 5 - 4+1} \times Z -$ $\frac{1}{2}\alpha = \frac{3}{5} Z - \frac{1}{2}\alpha = \frac{1}{10} Z = \frac{1}{10} \times \overline{abcd - abce}^2 +$ $\frac{1}{10} \times \overline{abcd - bcde}^2 +$ &c. which is a positive Number when the Roots are real Numbers.

Proposition II.

Let $x^n - Bx^{n-1} + Cx^{n-2} - \mathcal{D}x^{n-3} + Ex^{n-4} -$ &c. $\pm A = 0$ be an Æquation of any Degree, whose Roots with their Signs let be expressed by the Letters $a, b, c, d, e, f,$ &c. let M represent any Coefficient of this Æquation, L, N the Coefficients adjacent to M; K, O the Coefficients adjacent to L, N; I, P those adjacent to K, O; H, Q those adjacent to I, P, and so on. Let m represent the Exponent of M and let Z (as in the preceeding Proposition) represent the Sum of the Squares of all the possible Differences between the Terms of the Coefficient M. Then the Product of the Square of any Coefficient M multiply'd by the Fraction $\frac{1}{2} \times \frac{1}{1 - \frac{1}{n \times \frac{n-1}{2} \times \frac{n-2}{3} \times \&c. \times \frac{n-m+1}{m}}}$ doth always

always exceed $L \times N - K \times O + I \times P - H \times Q + \&c.$ by $\frac{\frac{1}{2} Z}{n \times \frac{n-1}{2} \times \frac{n-2}{3} \times \&c. \times \frac{n-m+1}{m}}$ which is always a positive Number, when the Roots a, b, c, d, e $\&c.$ are real Numbers positive or negative. Let the Æquation be of the seventh Degree or $x^7 - Bx^6 + Cx^5 - Dx^4 + Ex^3 - Fx^2 + Gx - A = o$, whose Roots let be a, b, c, d, e, f, g, in which Case $n = 7$. Let $M = E = abcd + abce + abcf + abcg + bcde + \&c.$ then $m = 4$, $L = -D$, $N = -F$, $K = C$, $O = G$, $I = -B$, $P = -A$, $Z = \overline{abcd - abce}|^2 + \overline{abcd - abcf}|^2 + \overline{abcd - abcg}|^2 + \&c.$ Therefore $\frac{1}{2} \times$

$1 - \frac{1}{7 \times \frac{6}{2} \times \frac{5}{3} \times \frac{4}{4}} \times E^2$ or $\frac{17}{35} E^2$ exceeds $D \times F - C \times G + B \times A$ by $\frac{\frac{1}{2} Z}{7 \times \frac{6}{2} \times \frac{5}{3} \times \frac{4}{4}}$ or $\frac{Z}{70} =$

$\frac{1}{70} \times \overline{abcd - abce}|^2 + \frac{1}{70} \times \overline{abcd - abcf}|^2 +$ $\&c.$

From this Propoſition, is deduced the following Rule for determining the Number of impoſſible Roots in any given Æquation. From each of the Unciæ of the middle Terms of that Power of a Binomial, whoſe

whoſe Index is the Dimenſions of the propoſed Æquation, ſubtract Unity, then divide each Remainder by twice the Correſpondent *Uncia*, and ſet the Fractions which reſult from this Diviſion, above the middle Terms of the given Æquation. And under any of the middle Terms if its Square multiplyed by the Fraction ſtanding above it, be greater than the Rectangle under the immediately adjacent Terms, *Minus* the Rectangle under the next adjacent Terms, *Plus* the Rectangle under the Terms then next adjacent — *&c.* place the Sign +, but if it be leſs, place the Sign —. And under the firſt and laſt Term place +. And there will be at leaſt as many impoſſible Roots, as there are Changes in the Series of the under-written Signs from + to —, or from — to +. Let it be required to determine the Number of impoſſible Roots in the Æquation $x^7 - 5x^6 + 15x^5 - 23x^4 + 18x^3 + 10x^2 - 28x + 24 = 0$. The *Unciæ* of the middle Terms of the 7th Power of a Binomial are 7, 21, 35, 35, 21, 7, from which ſubtracting Unity, and dividing each of the Remainders by twice the correſpondent *Uncia*, the Quotients will be $\frac{6}{14}, \frac{20}{42}, \frac{34}{70}, \frac{34}{70}, \frac{20}{42}, \frac{6}{14}$ or $\frac{3}{7}, \frac{10}{21}, \frac{17}{35}, \frac{17}{35}, \frac{10}{21}, \frac{3}{7}$ which Fractions place above the middle Terms of the Æquation, has

$$x^7 \overset{+}{-} \overset{\frac{3}{7}}{5}x^6 \overset{-}{+} \overset{\frac{10}{21}}{15}x^5 \overset{+}{-} \overset{\text{Bbbb}}{23}x^4 +$$

$23x^{4\frac{7}{3}} + 18x^{3\frac{7}{3}} + 10x^{2\frac{2}{1}} - 28x^{\frac{1}{7}} + 24 = 0$. Then
$\quad - \qquad + \qquad - \qquad + \qquad +$
becauſe the Square of $-5x^6$ multiply'd into the Fraction over its Head $\frac{3}{7}$, to wit $\frac{75}{7}x^{12}$ is leſs than $x^7 \times 15x^5$ or $15x^{12}$ I place the Sign $-$ under the Term $5x^6$. Becauſe the Square of $15x^5$ multiply'd by the Fraction over its Head $\frac{10}{21}$ to wit $\frac{705}{7}x^{10}$ is greater than $\overline{-5x^6 \times -23x^4} - \overline{x^7 \times 18x^3} = 97x^{10}$ I place the Sign $+$ under the Term $15x^5$. Seeing $\frac{8993}{35}x^8$ (the Square of the Term $-23x^4$ multiply'd by the Fraction over its Head $\frac{17}{35}$) is leſs than $15x^5 \times 18x^3 - \overline{-5x^6 \times 10x^2} + \overline{x^7 \times -28x} = 292x^8$, I place the Sign $-$ under the Term $23x^4$. Becauſe $\overline{18x^3}|^2 \times \frac{17}{35}$ or $\frac{5508}{35}x^6$ exceeds $\overline{-23x^4 \times 10x^2} - \overline{15x^5 \times -28x} + \overline{-5x^6 \times 24} = 70x^6$ I place the Sign $+$ under the Term $18x^3$. Since $\overline{10x^2}|^2 \times \frac{10}{21}$ or $\frac{1000}{21}x^4$ is leſs than $\overline{+18x^3 \times -28x} - \overline{-23x^4 \times 24} = 48x^4$ I place the Sign $-$ under the

the Term $10x^2$. Becauſe $\overline{28x}|^2 \times \frac{3}{7}$ or $336x^2$ is greater than $10x^2 \times 24 = 240x^2$ under $28x$ I place $+$, then under the firſt and laſt Terms I place $+$; and the ſix Changes of under-written Signs ſhews that there are ſix impoſſible Roots.

If the impoſſible Roots were to be found by the *Newtonian* Rule, the Operation would ſtand thus:

$$\begin{array}{ccccccccccc} & & \frac{1}{7} & & \frac{1}{9} & & \frac{3}{4} & & \frac{2}{3} & & \frac{5}{9} \\ x^7 & - & 5x^6 & + & 15x^5 & - & 23x^4 & + & 18x^3 & + & 10x^2 - \\ + & & - & & + & & + & & + & & + \end{array}$$

$$\begin{array}{ccccc} & & \frac{3}{7} & & \\ 28x & + & 24 & = & 0, \\ + & & + & & \end{array}$$

by which Rule there are found only two impoſſible Roots, whereas there are ſix to wit $1 + \sqrt{-3}$, $1 - \sqrt{-3}$, $1 + \sqrt{-2}$, $1 - \sqrt{-2}$, $1 + \sqrt{-1}$, $1 + \sqrt{-1}$, the ſeventh Root being -1.

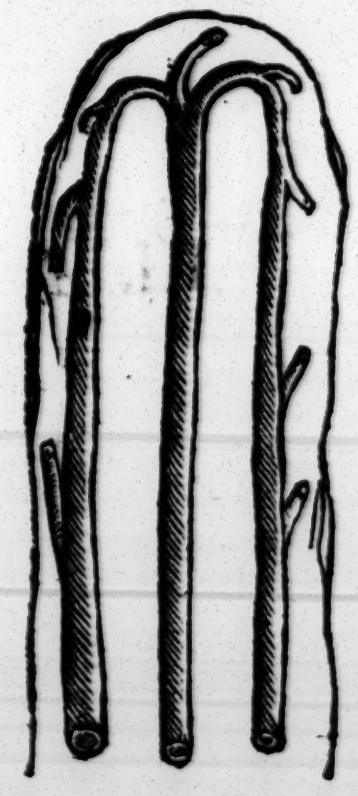
III. *A Letter from* Charles Price, *Esq;* of Trinity-Coll. Oxon, *relating to the Villi of the Stomach of Oxen, and the Expansion of the Cuticle through the Ductus Alimentalis. Communicated by Dr.* F. Nicholls, *F. R. S. and Præl. Anat.* Oxon.

Trinity-Coll. Oxon, July the 28th, 1728.

Dear SIR,

IN Pursuance of your Advice, I have spent some Part of this Summer in enquiring into the more intimate Constitution of the alimentary Passage of large Animals; in Hopes that the Analogy between them and human Subjects, may lead us into a more perfect Knowledge of our own Structure.

In the Stomach of the Cow I find two Things well worth observing: The first is, that the Villi composing the Villous Coat, (which are in Man so very small as to be scarce visible when examined separately) are in this Animal so very large, as to allow an exact Scrutiny into their Structure. Each Villus is formed by a Duplicature of the internal Lamina of the vascular Coat; from which it receives three Blood Vessels, as in the annex'd Figure, which represents one of the Villi of the Stomach of an Ox magnify'd. Whether or no the two Side-vessels are Arteries, and the Middle-vessel a Vein; and whether those small Branches arising from the Side-



vessels

veſſels are ſecretory Ducts carrying a Fluid from thoſe Arteries into the Cavity of the Stomach, making a Kind of Rivus perpetually running through the Ductus Alimentalis, I muſt leave others to judge.

The other Thing remarkable in the Stomachs of theſe large Animals is, that their internal Surface is covered by a Production of the Cuticle, which deſcends from the Lips quite through the alimentary Paſſage. I am induced to believe, that the Cuticle is continued through the Inteſtines as well in Man, as in large Animals; though its exceeding Fineneſs may make it leſs obſervable.

I have ſent you a Piece of the firſt Stomach of an Ox, in which the Veſſels running in the Villi are filled with Wax, and the Cuticle raiſed in Part; by which the above Particulars are ſufficiently proved.

If you think theſe Things new, and worth communicating to the *Royal Society*, I deſire you will communicate this to them in ſuch Manner as you think moſt proper: And that the Piece of Stomach (if worth the Acceptance of Sir *Hans Sloane*) may be preſented to him, with my beſt Reſpects, as an Acknowledgment of the Eſteem of,

SIR,

Your Humble Servant,

Ch. Price.

IV. *Obſervationes Aſtronomicæ habitæ in Obſervatorio* Bononienſi *Anno* 1727, *a Cl.* Euſtachio Manfredi, *R. S. S. Ex Epiſtola* J. Baptiſtæ Carbone *ad* Iſaacum de Sequeyra Samuda *M. D. Coll. Med. Lic. & R. S. S.*,

2. JAnuarii St. N. h. 9. 45′. 47″. Immerſ. 3^{i} Satellitis in umbram 4. Teleſcop. ped. 14. h. 11. 53′. 38″. ejuſdem Emerſio. Dub.

5. Jan. h. 6. 51′. 54″. Emerſ. 1^{i} Satell. Teleſc. pedum 11. Bononienſ. Clar.

7. Jan. h. 8. 54′. 12″. Emerſ. 1^{i} eodem Teleſc. Clariſſ.

7. Febr. h. 5. 50′. 5″. Imm. 3^{i} eodem Teleſc.
h. 7. 52′. 54″. Emerſ. ejuſdem Teleſc. ped. 14.

8. Febr. h. 8. 37′. 59″. Emerſ. 2^{i} Tel. ped. 11. aëre nonnihil nebuloſo.

21. Aug. h. 13. 34′. 39″. Imm. 1^{i} eodem Teleſc.

6. Sept. h. 11. 55′. 15″. Imm. 1^{i} eodem Teleſc.

17. Sept. h. 10. 48′. 59″. Imm. 3^{i} dub. eodem Teleſc.
h. 12. 40′. 30″. Emerſ. ejuſdem. ſubdub. eodem Tel.

9. Mart. h. 8. 50′. 6″. Emerſit ſpica ♍. a limbo Lunæ obſcuro.

Incipit

18. Sept. h. 0. 27′. 21″. Incipit Venus occultari poſt limbum Lunæ obſcurum.
h. 0. 28′. 13″. Immerſio totalis ♀.
h. 1. 16′. 45″. Incipit ♀ emergere è limbo ☾ lucido.
h. 1. 17. 50. Emerſit tota.
h. 1. 46. 24. Nunc limbus præcedens ☾ illuminatus præcedit ♀ in circulo horario 21″ temporis; & limbus Borealis ☾ item illuminatus eſt auſtralior ♀ 29″ temporis.

THE Second Plate at the Front of this Tranſaction *repreſents thirty-ſix Stones cut out of the Bladder by the Lateral Operation, as it is now improved by Mr.* Cheſſelden, *Surgeon to Her* Majeſty *and* St. Thomas's *Hoſpital, and* F. R. S. *who produced them before the* Society, *and at the ſame Time informed them, that the Perſon was ſixty-three Years of Age when he performed the Operation, and is notwithſtanding perfectly recovered.*

FINIS.

ADVERTISEMENT.

ALL Perſons concern'd in the Practice of Inoculating the Small-Pox, are deſired to keep a Regiſter of the Name, Age and Habitation of every Perſon inoculated, the Manner of the Operation, the Days of ſickening and of the Eruption, the Sort of Small-Pox that is produced, and the Event.

Where the true Small-Pox is not produced by Inoculation, it will be of Uſe to take particular Notice, whether the Patient had any other Kind of Eruption, what Symptoms preceded or attended it, whether the Inciſions inflam'd and run, and for what Time their Running continued.

In Caſe any Perſon ſhall happen to die after Inoculation, either in the Courſe of the Small-Pox, or after they are gone off, it is deſired that a particular Relation of the Caſe may be made, and atteſted, if it be judg'd neceſſary, by the neareſt Relations of the Party deceas'd, or by other credible Perſons, that were Witneſſes to the Fact.

They are intreated to ſend theſe Accounts, or an Extract from them, comprehending all Perſons inoculated from the Beginning to the End of the Years 1727 and 1728 to Dr. *Scheuchzer*, M. D. and F. R. S. at Sir *Hans Sloane*'s, Bart. in *Great Ruſſel-ſtreet* by *Bloomsbury-Square*, ſome Time in *January*, or at fartheſt in *February* next, that ſo the Reſult of them may be publiſh'd early in the Spring.

Nov. 8. 1728.

LONDON: Printed for WILLIAM INNYS, at the *Weſt End* of St. *Paul*'s.

M.DCC.XXVIII.

Jos: Banks

Numb. 405.

PHILOSOPHICAL TRANSACTIONS

FOR THE

Month of *NOVEMBER*, 1728.

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ERRATUM.

Pag. 554. *lin.* 26. *dele* è.

I. *The Barometrical Method of meaſuring the Height of Mountains, with two new Tables ſhewing the Hèight of the Atmoſphere at given Altitudes of Mercury. Extracted chiefly from the Obſervations of* John James Scheuchzer, *M. D. Profeſſor of Mathematicks at* Zurich, *and a Member of the Imperial, and Royal Societies of* London *and* Pruſſia. *By* J. G. Scheuchzer, *M. D. F. R. S. & Coll. Med. Lond. Lic.*

THE Height of Mountains, and their Elevation above the Level of the Sea, hath been at all Times thought worthy the Attention of inquiſitive Philoſophers. We find in *Pliny* *, that *Dicæarchus*, one of the old Geographers, a Diſciple of *Ariſtotle*, and, as *Pliny* himſelf ſtiles him, a Man of great Learning, had by particular Order of ſome Princes meaſured the Heights of ſeveral Mountains, and that the higheſt of them, *Mount Pelius* in *Theſſalia*, was found by his Obſervations 1250 Paces high perpendicularly. *Cleomedes* alſo, a *Grecian* Aſtronomer and Geographer, who lived ſometime before our Saviour's Nativity, aſſerts †, that the higheſt Mountain cannot be above 15 Stadia, or 9375 *Roman* Feet high.

But *Plutarch* ‡ fixes the perpendicular Height of the higheſt Mountains, as alſo the greateſt Depth of the Sea, only to 10 Stadia, or 6250 *Roman* Feet. It will appear by the Sequel of this Paper, that the

* Hiſt. Nat. L. xi. c. 65. † Cyclicæ Theor. Cap. x. ‡ In vita Aemilij.

Height of Mountains, as determined by theſe early Writers, doth not ſo very much deviate from Truth, as one would be apt to ſuſpect from the infant State of Arts and Sciences in thoſe Times. Particularly the 15 Stadia of *Cleomedes*, which make out 9375 *Roman*, or 10,214 *Paris* Feet, will be found by the following Obſervations to come very near the Height of the Mountains of *Swiſſerland*, which, although the higheſt of *Europe*, do not riſe above 10,000 *Paris* Feet above the Level of the Sea; and it may ſeem ſurprizing, that ſubſequent Writers, even ſuch as were otherwiſe deeply skill'd in mathematical Learning, have run them up to an extravagant, and altogether unnatural Height.

At firſt, it is not improbable, they went only upon bare Conjectures; but afterwards, when Geometry came to be more and more improved, Quadrants, Semicircles, and other Geometrical Inſtruments were call'd in Uſe, by the Means of which, and by a Trigonometrical Calculation, the Heights of Places could be determined in a more ſatisfactory Manner. And yet, however true the Principles be, upon which this Method is founded, however nice the Inſtruments, and however curious the Obſerver, the Method itſelf muſt be owned, and hath been found by undoubted Experiments, to fall far ſhort of that Accuracy, which it ſeems to promiſe; and the more conſiderable the Heights are, the more uncertain it will be. For in the firſt Place, as the State of the Air is very different in different Seaſons and different Weather, its Refraction alſo becomes thereby greatly altered, which occaſions the Tops of Mountains to appear higher at ſome Times than they do at others, and at all Times higher than they actually are. But beſides, there is another In-

conveniency,

conveniency, which whoever is acquainted with the true State of mountainous Countries, muſt needs be ſenſible of, and that is the extream Difficulty of meeting at the Bottom of high Mountains with Plains large enough for a proper horizontal Stand, or Baſis, to ſuch a Triangle, as an accurate and knowing Obſerver would think ſatisfactory to determine a conſiderable Height, making even proper Allowances for the Air's Refraction.

Among the many Improvements in Natural Philoſophy, which are owing to the Toricellian Tube, one of the moſt conſiderable Inventions of the laſt Century, it hath been thereby enriched with a new Method of meaſuring the reſpective Heights of Places, and their Elevation above the Level of the Sea; a Method, which, although it muſt be owned, that it hath not as yet, and perhaps, conſidering the Inconſtancy of the Air, hardly ever will be brought to an abſolute Degree of Certainty, is yet in many Reſpects preferable to the Trigonometrical one, as it hath alſo been found by Experience to come nearer the Truth, and leads us, by a new and ſingular Scale, from the very Horizon of the Sea to the Tops of the higheſt Mountains, a Diſtance far beyond the Reach of Geometrical Inſtruments. This new Method is grounded upon that eſſential Quality of the Air, its Gravity or Preſſure. As the Column of Mercury in the Barometer is counterpoiſed by a Column of Air of equal Weight, ſo whatever Cauſes will make the Air heavier or lighter, its Preſſure will be thereby increaſed, or leſſened, and conſequently the Mercury riſe or fall. Again the Air is more or leſs condenſed, or expanded, in Proportion to the Weight, or Force, which preſſes it: Hence it is, that

in *England*, *Holland*, the maritime Provinces of *France*, and in general all thoſe Countries which border upon the Sea, the Mercury ſtands higheſt, that the higher you remove from the Sea into the midland Countries, the lower the Mercury will deſcend, becauſe the Air alſo becomes more rarefied and lighter, and that upon the Tops of the higheſt Mountains it falls loweſt, and theſe Heights of the Mercury in different Places are reciprocally, as the Expanſions of the Air. From theſe Principles, ſupported by a competent Number of Obſervations, it hath been attempted by ſeveral learned Men, to derive proper Tables, whereby the Height of any Place may be determined, if the Height of the Barometer be given, or the Height of the Barometer determined from the given Altitude of the Place, and likewiſe the Expanſions of the Air ſettled, as they anſwer to every Inch, or Part of an Inch, in the Barometer.

I paſs over the firſt Experiment of this Kind, which was made in the Year 1648 (but a few Years after the Invention of the Torricellian Tube was made publick in *France* by Father *Merſenne*) by Monſieur *Perier*, according to the Directions of the celebrated Monſieur *Paſcal*, his Brother-in-Law, upon the high Mountain *Puy de Domme*, near *Clermont* in *Auvergne*, the Height whereof was thereby determined to 500 *French* Toiſes, or 3000 *Paris* Feet. (See the Appendix to *M. Paſcal*'s *Traitè de l' Equilibre des Liqueurs* *.) Nor will my preſent Purpoſe admit a particular Enumeration of thoſe made ſometime after, in 1661, 1665, and 1666, by *George Sinclair*, Profeſſor of Philoſophy in the Univerſity of *Glaſgow*, upon the Cathe-

* *Paris*, 1663, 8*vo*. *pag*. 177.

dral

dral of that Univerſity, upon ſeveral high Mountains in *Scotland*, and likewiſe in ſome Wells and Coal-pits, a particular Account whereof he inſerted in his *Ars magna gravitatis & levitatis* *. I will only obſerve, that theſe Experiments of *Sinclair*, as well as that of Monſieur *Perier*, were intended not ſo much to lay the Foundation of a Calculation, whereby to determine the differing Heights of Places, as to prove the Gravity and Preſſure of the Air, a Problem very much controverted at that Time, and to ſhew, that the ſame is much more conſiderable in Valleys than at the Top of Mountains, and ſtill greater in Proportion at the Bottom of Wells, Mines, *&c.*

But this Matter was purſued ſtill farther by the Members of the Royal Academy of Sciences at *Paris*, particularly, when by Order of *Lewis* XIV, they drew that expenſive *Meridian* Line acroſs the whole Kingdom of *France*. *M. Mariotte*, a celebrated Member of that Academy, was one of the firſt that laid down certain Rules for the Conſtruction of ſuch Tables, as might ſerve to determine both the Elevation of Places above the Level of the Sea from given Altitudes of Mercury, and the Heights of the Air, anſwering to every Line of Mercury in the Barometer, from 28″, where the Mercury was ſuppoſed to ſtand at a Medium near the Sea. The Principles he went upon, and the Method he followed, he diſcourſed of at large, in his *Second Eſſay de la Nature de l' Air*.

Sometime after, in 1686, the ingenious Dr. *Edmund Halley* went about another Calculation, which he derived partly from Principles agreeing with thoſe of

* Roterodami, 1669, 4*to*. *pag*. 129, 132, 134, 144, & ſeq.

M. Ma-

M. Mariotte, partly from the ſpecifick Weight of Air and Mercury, which were found by Experiments to be as 1 to 10,800; Air being to Water as 1 to 800, and Water to Mercury as 1 to 13 $\frac{1}{2}$, or very near it. If ſo, as the Column of Mercury in the Barometer is counterpoiſed by a Column of Air of equal Weight, a Cylinder of Air of 10,800 Inches, or 900 Feet will be equal to one Inch of Mercury, and 90 Feet to $\frac{1}{10}$ of an Inch, or 75 to $\frac{1}{12}$ Part of it. The Height of the Air, as it anſwers to one Inch of Mercury, being thus determined, and the Expanſions of the Air being reciprocally as the Heights of Mercury, Dr. *Halley*, by the Help of the Hyperbola and its Aſymptotes, calculated two Tables, one ſhewing the Altitude to given Heights of Mercury, the other the Heights of Mercury at given Altitudes. Theſe Tables, the firſt that ever were calculated, together with the Doctor's whole Method of proceeding, and an ingenious Attempt of his to diſcover the true Reaſon of the Riſe and Fall of Mercury upon Change of Weather, were printed in the *Philoſophical Tranſactions* *, and the Tables themſelves were very lately re-printed, with ſome Obſervations upon them, by Dr. *Deſaguliers* †.

In the Year 1703, when the *Meridian* Line, firſt begun by *M. Picard* in 1669, afterwards continued in 1683, was farther purſued, ſeveral Obſervations of this Kind were made, and the Heights of ſeveral conſiderable Mountains, particularly in the Southern Parts of *France*, determined as well by Trigonometrical as Barometrical Obſervations. Monſieur *Caſſini* the Younger took that Opportunity to compare theſe Obſervations

* N° 181. *pag.* 106. † Phil. Tranſact. N° 386.

with

with the Rules laid down by *M. Mariotte* *, in order to which, and conform to the ſaid Rules, he calculated two Tables, one ſhewing the Height of the Atmoſphere, as it anſwers to every Line of Mercury in the Barometer, the other determining the Height of the Atmoſphere above the Level of the Sea at given Altitudes of Mercury. But having afterwards, upon Compariſon, found that the Obſervations made in 1703, did not in the Main agree with the Rules of *M. Mariotte*, and that the Heights of Places, as they appeared by thoſe Obſervations, exceeded, generally ſpeaking, the Numbers reſulting from the Tables made by him according to the ſaid Rules, he thought it neceſſary to calculate two new ones, wherein indeed the Reſults are conſiderably greater than in the Tables framed according to the Rules of *M. Mariotte*; inſomuch, that for Inſtance, a Place, where the Mercury falls to 22 Inches, riſes above the Level of the Sea, according to *Mariotte*, 852 Toiſes, or 5112 *Paris* Feet; and, according to *Caſſini*, 1158 Toiſes, or 6948 Feet, which makes a Difference of 1836 *Paris* Feet, or 306 Toiſes. Dr. *Deſaguliers*, in his Diſſertation concerning the Figure of the Earth †, hath already ſhewn how far the Obſervations made by the Gentlemen, that drew the *Meridian* acroſs the Kingdom of *France*, differ from each other; inſomuch, that there are not two in nine, where the Number of Toiſes, ſaid to correſpond to the Heights of the Barometer, agree together; and that conſequently the Heights of Mountains, as determined by theſe Obſervations, are little to be depended on.

* Memoires de l' Acad. Royale, 1705. *pag.* 61. & ſeq.

† Phil. Tranſ. N° 386. *pag.* 211.

My

My Father, Dr. *J. J. Scheuchzer*, in his Journeys over the Mountains of *Swisserland*, as they were more particularly calculated for the Improvement of Natural Philosophy in its several Branches, neglected no Opportunity, along with his other Observations, to make such Experiments with the Barometer, as might serve to illustrate the Qualities of the Air, to settle the respective Heights of Places, and particularly to shew, how much our Mountains rise, as well above the Level of the Sea, as above other neighbouring Mountains in *France*, *Italy*, *Spain*, &c. Many of these Observations are scattered up and down in his Writings, particularly his *Itinera Alpina*, and the several Parts of his *Natural History of Swisserland*, which last Work was published in *High German*. It would be too tedious to mention all the Experiments he made at different Times, and upon different Mountains. But my Design in this Paper requires me to be particular in one, which for the Height measured both with the Line and Barometer is, I believe, the most considerable that ever was made, and which enabled him more particularly to examine the two Tables made by *Cassini* the Younger, according to the Rules of Mr. *Mariotte*, and the Observations made by him and others, when the *Meridian* Line was perfected in 1703.

This curious Experiment was made in the Year 1709, at *Pfeffers*, a celebrated Mineral Water in the County of *Sargans*, at the Bottom and Top of a Mountain, which rises from a small Brook, called the *Taminna*, to the Height of 714 *Paris* Feet, as appeared by letting a Line drop down perpendicularly from a Tree at Top full to the Bottom. At the Bottom of this Mountain, near the *Taminna*, the Mercury was by repeated Experiments

periments obſerved at 25″, 9⅓‴, and at the Top it deſcended to 24″, 11⅓‴, ſo that it fell juſt 10 Lines, for 714 Feet, which gives about 71 *Paris* Feet for a Line, if the Heights anſwering to every Line were ſuppoſed to be equal.

I muſt here once for all deſire the Reader to take Notice, that I have made uſe in this Paper of *Paris* Meaſure, namely, of Toiſes (°) Feet (′) Inches (″) and Lines (‴). Every Toiſe is reckoned at ſix Foot, the Foot is divided into twelve Inches, and the Inch into twelve Lines.

The Heights of the Barometer at the Bottom and Top of the Mountain being thus given, the Height of it ſhould be, according to *M. Mariotte*, 116°, 0′, 8″, 11‴, or 696 *Paris* Feet, 8″, 11‴, which falls 17′, 3″, 1‴, ſhort of the true Height, and according to *Caſſini* 153° 3′, 8″, that is, 921 *Paris* Feet, 8″, which exceeds the true Height by 207 *Paris* Feet, 8 Inches; whereby it appears, that the Table made according to the Rules of *Mariotte* is much preferable to that of *Caſſini* the Younger. The ſame was likewiſe confirmed by another Experiment made in *June* 1715, upon the Steeple of our Cathedral at *Zurich*. At the Foot of the Steeple the Barometer ſtood at 26″, 10‴, and at the Top at 26″, 7½‴, and the Height of the Steeple was found by the Line of 241 *Paris* Feet, 4 Inches, which gives very near 69 *Paris* Feet for one Line. According to the Table of *Mariotte*, the Height of the Steeple ſhould have been of 237 *Paris* Feet, according to *Caſſini*, 265, and according to the new Calculation (of which by and by) made purſuant to the Experiments above, it comes to 243°, 16″, 2‴, or about two Foot more than the true Height.

It appearing by the Experiments made at *Pfeffers*, that from 25″, 9⅓‴ the Barometer deſcends to 24″, 11⅓‴, that is, juſt 10 Lines, for the Height of 714 Feet, and the Expanſions of the Air being reciprocally as the Heights of Mercury, my Uncle, Dr. *John Scheuchzer*, undertook, purſuant to theſe Principles, and the Properties of the Hyperbola, to calculate a new Table, after the following Method.

As the Difference of the Logarithms of the two given Heights of the Barometer 25″ 9⅓‴ and 24″ 11⅓‴, that is 309⅓ and 299 ¹, or	Is to Foot,	So the Difference of the Logarithms of the Height of Mercury near the Sea, 28″ 1‴ to any leſſer Height, as for Inſtance 28″ 0‴, that is 337 — 336, or	To the Height of the Atmoſphere above the Level of the Sea, as it anſwers to one Line of Mercury, is
928 — 898 142717	714	1011 — 1008 12906	64′, 6″, 9‴

Thus the Height of the Atmoſphere at 28″ appears to be of 10°, 4′, 6″, 9‴, but, according to *Mariotte*, it is only of 10°, 3′, or 63 Feet, and *Caſſini* ſuppoſes it only at 10°, or 60 Foot.

In like Manner the Height of the Atmoſphere, from 28″, 0‴, to 27″, 11‴ is found to be 64′, 9″, 2‴. According to the ſame Rule half the Height of the Atmoſphere, that is, the Height of the Place, where the Mercury in the Barometer would deſcend to 14 Inches, appears to be, 15060′, 3″, 0‴, or 2510°, 0′, 3″, 0‴. Still upon the ſame Principle the Mercury will deſcend to one Line at the Height of 133,397 *Paris* Feet above the Level of the Sea, which make 22,232 Toiſes,

5 Feet,

0	1030	3	9	10	—	1
0	1044	0	2	6	21	0
1	1058	2	11	3	—	11
3	1073	0	0	1	—	10

TABLES of the Height of the ATMOSPHERE to given Altitudes of *Mercury*.

The Fall of *Mercury* in the Barometer		The Height of the Atmosphere, as it answers to every Line in the Barometer, according to *Mariotte*.				According to *Cassini*.		According to Dr. *Scheuchzer*.			
II	III	o	I	II	III	o	I	o	I	II	III
0	0	10	3	0	0	10	0	10	4	6	9
	1	10	3	2	3	10	1	10	4	9	2
	2	10	3	4	6	10	2	10	4	11	5
	3	10	3	6	10	10	3	10	5	1	1
	4	10	3	9	1	10	4	10	5	4	1
	5	10	3	11	4	10	5	10	5	6	6
	6	10	4	1	9	11	0	10	5	8	11
	7	10	4	4	1	11	1	10	5	11	3
	8	10	4	6	5	11	2	10	0	1	8
	9	10	4	8	10	11	3	11	0	4	1
	10	10	4	11	2	11	4	11	0	6	6
	11	10	5	1	7	11	5	11	0	8	11
1	0	10	5	4	0	12	0	11	0	11	4
	1	10	5	6	5	12	1	11	1	1	10
	2	10	5	8	10	12	2	11	1	4	4
	3	10	5	11	1	12	3	11	1	6	10
	4	11	0	1	9	12	4	11	1	9	5
	5	11	0	4	3	12	5	11	2	0	0
	6	11	0	6	9	13	0	11	2	2	7
	7	11	0	9	3	13	1	11	2	5	2
	8	11	0	11	10	13	2	11	2	7	9
	9	11	1	2	4	13	3	11	2	10	4
	10	11	1	4	11	13	4	11	3	1	0
	11	11	1	7	7	13	5	11	3	6	4
2	0	11	1	10	2	14	0	11	3	9	0
	1	11	2	0	9	14	1	11	3	11	8
	2	11	2	3	4	14	2	11	4	2	4
	3	11	2	6	0	14	3	11	4	5	0
	4	11	2	8	8	14	4	11	4	7	8
	5	11	2	11	4	14	5	11	4	10	4
	6	11	3	2	1	15	0	11	5	1	0
	7	11	3	4	10	15	1	11	5	3	9
	8	11	3	7	7	15	2	11	5	6	7
	9	11	3	10	4	15	3	11	5	9	5
	10	11	4	1	1	15	4	12	0	0	5
	11	11	4	3	11	15	5	12	0	3	5
3	0	11	4	6	9	16	0	12	0	6	6
	1	11	4	9	6	16	1	12	0	9	8
	2	11	5	0	4	16	2	12	1	0	10
	3	11	5	3	3	16	3	12	1	4	0
	4	11	5	6	2	16	4	12	1	7	2
	5	11	5	9	1	16	5	12	1	10	4
	6	12	0	0	0	17	0	12	2	1	6
	7	12	0	2	11	17	1	12	2	4	8
	8	12	0	5	11	17	2	12	2	8	0
	9	12	0	8	11	17	3	12	2	11	2
	10	12	0	11	11	17	4	12	3	2	4
	11	12	1	2	11	17	5	12	3	5	8
4	0	12	1	6	0	18	0	12	3	8	10
	1	12	1	9	1	18	1	12	4	0	0
	2	12	2	0	2	18	2	12	4	3	2
	3	12	2	3	3	18	3	12	4	6	4
	4	12	2	6	5	18	4	12	4	9	6
	5	12	2	9	7	18	5	12	5	0	8
	6	12	3	0	9	19	0	12	5	3	10
	7	12	3	3	11	19	1	12	5	7	0
	8	12	3	7	2	19	2	12	5	10	2
	9	12	3	10	5	19	3	13	0	1	4
	10	12	4	1	9	19	4	13	0	4	6
	11	12	4	5	0	19	5	13	0	7	8
5	0	12	4	8	4	20	0	13	0	10	10
	1	12	4	11	8	20	1	13	1	2	3
	2	12	5	3	0	20	2	13	1	5	8
	3	12	5	6	5	20	3	13	1	9	1
	4	12	5	9	10	20	4	13	2	0	6
	5	13	0	1	4	20	5	13	2	4	2
	6	13	0	4	9	21	0	13	2	7	10
	7	13	0	8	3	21	1	13	2	10	6
	8	13	0	11	10	21	2	13	3	3	2
	9	13	1	3	4	21	3	13	3	6	10
	10	13	1	6	11	21	4	13	3	10	6
	11	13	1	10	6	21	5	13	4	2	2
6	0	13	2	2	2	22	0	13	4	5	10
	1	13	2	5	10	22	1	13	4	9	6
	2	13	2	9	6	22	2	13	5	1	2
	3	13	3	1	3	22	3	13	5	4	10
	4	13	3	5	2	22	4	13	5	8	6
	5	13	3	8	9	22	5	14	0	0	2
	6	13	4	0	7	23	0	14	0	4	2
	7	13	4	4	5	23	1	14	0	8	3
	8	13	4	8	3	23	2	14	1	0	4
	9	13	5	0	1	23	3	14	1	4	5
	10	13	5	4	0	23	4	14	1	8	6
	11	13	5	8	0	23	5	14	2	0	7
7	0	14	0	0	0	24	0	14	2	4	8
	1	14	0	4	0	24	1	14	2	8	9
	2	14	0	8	0	24	2	14	3	0	10

The Height of the Atmosphere above the Level of the Sea, according to *Mariotte*.				According to *Cassini*.		According to Dr. *Scheuchzer*.				Height of the Mercury in the *Barometer*.	
o	I	II	III	I	II	o	I	II	III	II	III
0	0	0	0	0	0	0	0	0	0	28	0
10	3	2	3	10	1	10	4	9	2	—	11
21	0	6	9	20	3	21	3	8	7	—	10
31	4	1	7	31	0	32	2	9	8	—	9
42	1	10	8	41	4	43	2	1	9	—	8
52	5	10	0	52	3	54	1	8	3	—	7
63	3	11	9	63	3	65	1	5	2	—	6
74	2	3	10	74	4	76	1	4	5	—	5
85	0	10	3	86	0	87	1	6	1	—	4
95	5	7	1	97	3	98	1	10	2	—	3
106	4	6	3	109	1	109	2	4	8	—	2
117	3	7	10	121	0	120	3	1	7	—	1
128	2	11	10	133	0	131	4	0	11	27	0
139	2	6	3	145	1	142	5	2	9	—	11
150	2	3	1	157	3	154	0	7	1	—	10
161	2	2	5	170	0	165	2	1	11	—	9
172	2	4	2	182	4	176	3	11	4	—	8
183	2	8	5	195	3	187	5	11	4	—	7
194	3	3	2	208	3	199	2	1	11	—	6
205	4	0	5	221	4	210	4	7	1	—	5
216	5	0	3	235	0	221	1	2	10	—	4
228	0	2	7	248	3	233	4	2	2	—	3
239	1	7	6	262	1	245	1	3	2	—	2
250	3	3	1	276	0	256	4	9	6	—	1
261	5	1	3	290	0	268	2	6	6	26"	0
273	1	2	0	304	1	280	0	6	2	—	11
284	3	5	4	318	3	291	4	8	6	—	10
295	5	11	4	333	0	303	3	1	6	—	9
307	2	8	0	347	4	315	1	9	2	—	8
318	5	7	4	362	3	327	0	7	6	—	7
330	2	9	5	377	3	338	5	8	6	—	6
342	0	2	3	392	4	350	5	0	5	—	5
353	3	9	10	408	0	362	4	7	0	—	4
365	1	8	2	423	3	374	4	4	5	—	3
376	5	9	3	439	1	386	4	4	10	—	2
388	4	1	2	455	0	398	4	8	3	—	1
400	2	7	11	471	0	410	5	2	9	25	0
412	1	5	5	487	1	423	0	0	5	—	11
424	0	5	9	503	3	435	1	1	3	—	10
435	5	9	0	520	0	447	2	5	3	—	9
447	5	3	2	536	4	459	4	0	5	—	8
459	5	0	3	553	3	471	5	10	9	—	7
471	5	0	3	570	3	484	2	0	3	—	6
483	5	3	2	587	4	496	4	4	11	—	5
495	5	9	1	605	0	509	1	0	11	—	4
508	0	6	0	622	3	521	4	0	1	—	3
520	1	5	11	640	1	534	1	2	5	—	2
532	2	8	10	658	0	546	4	8	1	—	1
544	2	3	10	676	0	559	2	4	11	24	0
556	5	11	11	694	1	572	0	4	11	—	11
569	2	0	1	712	3	584	4	8	1	—	10
581	4	3	4	731	0	597	3	2	5	—	9
594	0	9	9	749	4	610	1	11	11	—	8
606	3	7	4	768	3	623	1	0	7	—	7
619	0	1	1	787	3	636	0	4	5	—	6
631	4	0	0	806	4	648	5	11	5	—	5
644	1	7	2	826	0	661	5	9	7	—	4
656	5	5	7	845	3	674	5	10	11	—	3
669	3	7	4	865	1	688	0	3	5	—	2
682	2	0	4	885	0	701	0	11	1	—	1
695	0	8	8	905	0	714	1	9	11	23	0
707	5	8	4	925	1	727	3	0	2	—	11
720	4	11	4	945	3	740	4	5	10	—	10
733	4	3	9	966	0	754	0	2	11	—	9
746	4	3	7	986	4	767	2	3	5	—	8
759	4	4	11	1007	3	780	4	7	7	—	7
772	4	9	8	1028	3	794	1	3	5	—	6
785	5	5	11	1049	4	807	4	1	11	—	5
799	0	5	9	1071	0	821	1	5	1	—	4
812	1	9	5	1092	3	834	4	11	11	—	3
825	3	4	0	1114	1	848	2	10	5	—	2
836	5	2	6	1136	0	862	1	0	7	—	1
852	1	4	8	1158	0	875	5	6	5	22	0
865	3	10	0	1180	1	889	4	4	11	—	11
879	0	8	0	1202	3	903	3	6	1	—	10
892	3	9	3	1225	0	917	2	10	11	—	9
906	1	2	5	1247	4	931	2	7	5	—	8
919	4	11	2	1270	3	945	2	7	7	—	7
932	2	11	9	1293	3	959	2	11	9	—	6
947	1	4	2	1316	4	973	3	8	0	—	5
961	0	0	5	1340	0	987	4	8	4	—	4
974	5	0	6	1363	3	1002	0	0	9	—	3
988	4	4	6	1387	1	1016	1	9	3	—	2
1002	4	0	6	1411	0	1030	3	9	10	—	1
1016	4	0	6	1435	0	1044	0	2	6	21	0
1030	4	4	6	1459	1	1058	2	11	3	—	11
1044	5	0	6	1483	3	1073	0	0	1	—	10

6		11	13	5	8	0	23	5	14	2	0	7		1002	4	0	6	1411
6	7	0	14	0	0	0	24	0	14	2	4	8		1016	4	0	6	1435
6		1	14	0	4	0	24	1	14	2	8	9		1030	4	4	6	1459
		2	14	0	8	0	24	2	14	3	0	10		1044	5	0	6	1483

5 Feet, or 11 *Paris* Miles (at 2000 Toiſes the Mile) 232 Toiſes, 5 Foot. But as in order to determine the whole Height of the Atmoſphere, the Logarithm of 1‴ ought to be deducted from the Logarithm of 336‴ or 28″ 0‴, and as that Logarithm is 00000, it follows from thence, that beyond the Place, where the Mercury would deſcend to 1‴, the Air is expanded into an Indefinite Space.

For the Satisfaction of the Curious, I have added the Tables themſelves, to wit, thoſe which *Caſſini* the Younger calculated according to the Rules of *Mariotte*, thoſe which he deduced from the Obſervations made by the Gentlemen of the Royal Academy of Sciences, who drew the *Meridian* Line, and thoſe which my Uncle calculated from the Obſervation made at *Pfeffers* in 1709.

In another Paper on this Subject I intend to compare the Height of Mountains, as determined by divers antient and modern Writers, with the true Height of them, as it appears to be by the Barometrical Obſervations, particularly thoſe made by my Father on the high Mountains of *Swiſſerland*.

H. *Obſervations of a Difference of Sex in* Miſleto, *in a Letter from the Reverend Mr.* Edmund Barrel *to Sir* Hans Sloane, *Bart.* &c.

SIR,

AFTER I had mentioned my being pretty well aſſured, that the Plants of Miſleto were ſome of them Male, and ſome Female; and had promiſed to

communicate to you, ſuch farther Obſervations as I ſhould make of that Matter; I was diſcouraged from giving you any Trouble about it, by an Information I received, that the *eminently learned Dr. Herman Boerhave* had already told the World of a Difference of Sex in Miſleto. But having, ſince that, ſeen the *Hiſtoria Plantarum*, which is publiſhed as of his dictating; I find, that he mentions it in ſuch a Manner as makes me ſuſpect, that he only took his Notion from *Tournefort*, and was not fully apprized of the true Nature of theſe Plants. *Ovarium alio à Flore loco natum* ſeems to ſuppoſe both Flower and Ovary to be on the ſame Plant, though in *diſtinct Places* of that Plant. I have therefore now written down my Obſervations for your Peruſal in the ſame Order that I made them.

I have (from my own ſowing of the Berries) four thriving Plants of Miſleto growing on one Tree in my Garden. Theſe, being often in my View, gave me the firſt Apprehenſion, of there being any Difference of Kind, or Sex, in this Shrub. They were not of Age to bear Flower or Fruit till 1726; when one of them bore a Berry or two; and expecting that they ſhould all do ſo the following Year, I frequently examined them, and found that two Plants had Berries, and two had none. I then went and examined the Miſleto on other Trees, which have Plants of above 20 Years Growth. And I find the Method of Nature to be thus.

Dr. *Grew* obſerves, that many Plants make a viſible Preparation in the former Year for the Flower and Fruit of the next Seaſon. This is done by Miſleto. At the latter End of *May*, the Male Plants put out little

little Knobs, at the Joints and Tops of their Boughs; which at firſt are not very unlike the young green Berries; but they ſoon appear evidently diſtinct from them, and being by the latter End of *July*, grown as large as the Berries, are then not at all like them; ſpreading wider upwards, and having 3, or 4, or 5 *Buds*, at the Top of each Knob. About *June*, the Female Plant alſo makes a like Preparation; putting out at the Joints and Tops of the Boughs, Knobs, which are more ſharp, and ſhorter than thoſe of the Male; with 1, or 2, but moſt commonly with 3 *Buds*, or ſmall Points at the Top of each Knob. I call them *Buds*, becauſe in their Seaſon they open into Flowers, both in the Male and Female Plants; all the reſt of the Knob ſerving only for Footſtalks to the Flowers, in the one Sort, and to both Flower and Fruit in the other. By the latter End of *Auguſt* the Berries are grown much larger than the Knobs on the Male Plants. And from thence, till late in *January*, there is little worth Remark in either Plant; only the Berry grows ſomewhat bigger, and becomes ripe; and the Knobs on the Male grow more and more yellow; ſo that one may, at that Time, diſcern a Male from a Female Plant, at a conſiderable Diſtance. By the 20th of *February* Miſſeto is in Bloom, both Male and Female. The Knobs of the Male are open at the Top with 3, or 4, or 5 Bloſſoms; which are very well deſcribed (though in ſhort) in *Boerhave*'s *Hiſtoria Plantarum*.

The Female Plant flowereth alſo now, with a Bloſſom (which *Boerhave* calls the Ovarium) exactly like the Male Flower; ſave only, that the whole Female Flower is not bigger than one Leaf of the Male

Flower. They both continue in full Bloom till the Middle of *March*, when the Male Bloſſoms begin to wither and drop off. And by the 20th of *March* the young Berries begin to ſhew themſelves, ſwelling forth, one under each Female Bloſſom; which often adheres to the Top of the Berry; and being carried up with it, preſently withers, and ſoon falls off again; tho' ſome continued on till the 12th of *May*, when the Berries were of the Size of a great Pin's Head.

This compleated the Year's Obſervation. And I think it is much to be wondered at, that this Plant, which hath been the Admiration of all Ages, ſhould (ſcarce ever) find one Obſerver ſo curious as to follow the Changes of it, through one whole Year's Revolution. For if this had been done with any Accuracy, it muſt have been very evident, that one Sort of Miſleto was very different from the other: One Sort bearing very ſmall Flowers, with Berries ſucceeding them: the other bearing much larger Flowers, not ſucceeded by any Berries; the very Footſtalk of the Male falling off with the Flower; whereas the Footſtalk of the Female, becomes a Footſtalk to the Berry. It is poſſible, that this Difference of the Sex, in the Plants of Miſleto, may be of Conſequence in Medicine: To thoſe therefore, who would make any Experiments of the different Virtues of theſe Plants, I offer this *general Obſervation*: That there is no Time of the Year wherein the Difference of theſe two Sorts, or Sexes of Miſleto, is not very eaſy to be ſeen and known, by the Marks I have mentioned: And the meaneſt Herb-woman will ſoon have Skill enough to bring the Sort they are ordered to procure; there being as great

great a Plenty of the Male Plants as there is of the Female. I follow your Commands, rather than my own Inclinations, in giving you this Trouble, and am, with the greateſt Reſpect,

S I R,

Your moſt obliged, and

Sutton, near *Dartford*,
Auguſt 20, 1728.

moſt obedient humble Servant,

Edmund Barrell.

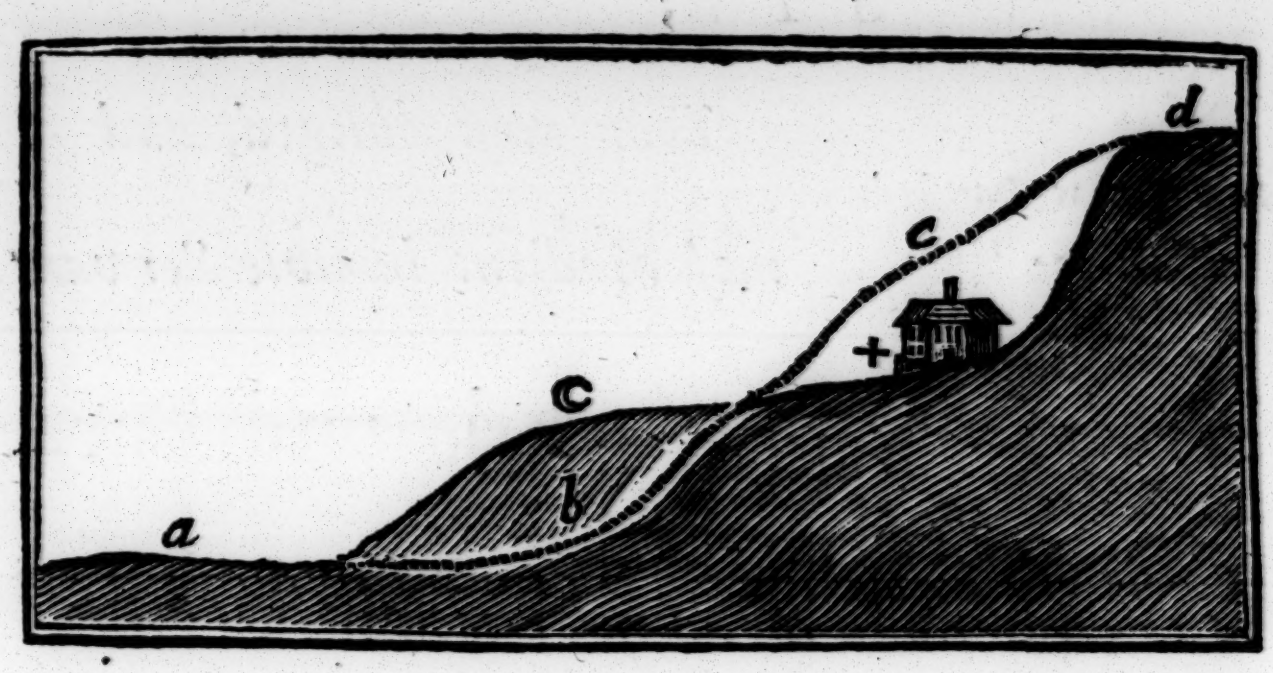
III. *An uncommon Sinking of the Ground in* Kent. *Communicated in a Letter to Mr.* Peter Collinſon.

S I R,

IT is within this three or four Days, that I have firſt been able to get a more particular Account of the ſinking of the Lands at *Lymne* in *Kent*, and even now, perhaps, it will be but an imperfect one.

It is now about two Years ſince it happened, and was the Conſequence of a very wet Seaſon, when the Waters, that had fallen on the Up-lands, and were not carried off by Drains, ſoaked into the Ground in ſuch Quantities as to form a quick Sand at ſome conſiderable Depth in the Earth (at leaſt this is what we look on to have cauſed the Phænomenon) which not being able to bear the Weight upon it, broke out at the Side of the Hill,

Hill, and raiſed the lower Parts of it ; letting the Brow ſink 40 or 50 Foot, as I gueſs. I am but a rude Deſigner, but can truſt you ſo far as to give ſuch a Sketch as I can draw; for, perhaps, you may underſtand me the better for it.



a b c d the Profile of the Land.

a the flat Land at Bottom 3 or 4 Mile from the Sea.

d the flat Land at Top, ſtiff Ground and rocky.

* The Place of the Farm at preſent, which not only ſunk down from *d* 40 or 50 Foot, but was alſo moved ſomewhat towards *a*.

b the lower Part raiſed to C.

The Ground ſunk in a Night, and was not perceived by the Farmer's Family till they found the Change in the Morning, by their Door-caſes not ſuffering the Doors to open. The Houſe is ſtrangely rent by this Accident, and, had it not been Timber built, muſt have fell, (as a mighty ſtrong Barn near it did, which was built of Stone) for one great Crack of the Earth went through the Middle of it, and ſplit a large Kitchen Chimney from Top to Bottom.

IV. *Obſer-*

IV. *Obſervationes Aſtronomicæ* Pekini *habitæ à* R. P. Ignatio Kögler Soc. Jeſu Tribun, Math. *in* Sinis *Præſide. Ex Epiſtolâ* R. P. Joh. Bapt. Carbone *ad* Iſaacum de Sequeyra Samuda, R. S. S. *&c.*

St. Novo, 1724. H. ′ ″

NOV. 5. Satelles 3us. immerſus eſt in 4 umbram. - -	6	9	0 p. m.
Nov. 20. Satelles 2us. prodiit ex umbra 4 - - - -	6	44	0 Veſp.
Nov. 30. Sat. 1us. ex 4 umbra emerſit.	6	14	0 Veſp.
Dec. 23. Emerſio Satell. j. ex umbra 4.	6	19	0 Veſp.

1725.

Martij die 11. circa hor. 9. matut. ⊙ inter tenues nubes allucens, & colorato halone cinctus, in eo ad dexteram & lævam duos parhelios efformabat multùm reſplendentes. Duravit ſpectaculum perſe mihoram.

Maij 10. hor. 4. manè, diſt. Jovis à ✶ ♀ ♒ 9′ 5″. 4 erat ad occaſum; eſtque diſtantia computanda à centro. Die 11. eâdem horâ, 4 jam prætergreſſus ✶ ♀ ab ea diſtabat ad ortum boreum 1′ 10″ ſcil. a centro. hor. 5, diſtantiâ centrorum 1′ 36″

Die 12. hor. 4. diſt. 4 à ✶ ♀ 10′ 10″

Die 13. eadem hor. 19′ 50″

	H.	′	″
Jun. 23. Satelles 3. ſubiit Jovis Umbr.	2	29	0
Julij 9. Sat. 1us. in 4 Umbram	2	55	30
Aug. 9. Imm. Satell. 1. in 4 Umbr.	11	27	0 Veſp.

Aug. 31. Satell. 1 & 2us. 4 in ♂ proxima penè in unum coaleſcebant. Non potuit diſcerni quiſnam prior 4. ſubin umbram ſubingreſſus ſit, Ille duorum Satellitum in unum coaleſcentium penitus diſparuit - -	H.	1	11	
	11	45	0	
Sept. 19. Emerſ. Sat. 1. ex 4 Umbra	6	51	30	Veſp.
Oct. 2. Emerſ. Sat. 1. ex 4 Umbra	10	45		Veſp.
Oct. 10. Idem Satell. 1. emerſit -	12	42		Man.
Oct. 11. Ejuſdem Emerſio - -	7	9		Veſp.
Oct. 15. Satell. 3us. prodiit a tergo Jovis - - -	6	46		Veſp.
Dein diſparuit in Umbr. 4 -	7	4		
Tandem ex eâdem Umbra emerſit	10	20		
Oct. 19. Satellitis 1. Emerſio -	9	9		Veſp.
Oct. 20. Satel. 2us. emerſit ex 4 Umbr.	9	6		Veſp.
Oct. 25. Emerſ. 1. Sat. ex 4 Umbr.	11	6		Veſp.
Oct. 27. Emerſ. 2. Satell. ex Umbr. 4	11	45	30	Veſp.
Nov. 3. Emerſ. 1. Satell. - -	7	27	40	Veſp.
Nov. 19. Ejuſdem Sat. 1. emerſ. -	5	42	30	Veſp.
Nov. 20. Satell. 3us. cœpit emergere ex 4 Umbr. - - -	6	26	30	Veſp.

Eclipſis totalis Lunæ, die 22 Octob. ineunte à media nocte.

		H.	1	11
Margo Umbræ attigit	Initium veræ Umbræ è proximè Nod. - - -	0	49	0
	Grimaldum - - -	0	51	0
	Ariſtarchum - - -	0	55	30
	Keplerum - - -		59	0
	Mare Humorum - - -	1	2	0

Gaſſendum

		H.	′	″
Margo Umbræ attigit	Gaſſendum - - -	0	3	0
	Sinum Irid. & Morinum - -	0	5	0
	Copernicum - - -	0	6	30
	Bullialdum - - -	0	9	30
	Eratoſthenem - - -	0	11	0
	Platonem - - -	0	54	0
	Tychonem - - -	0	59	0
	Aratum, toto Tych. obtecto -	0	59	30
	Manilium - - -	0	22	0
	Menelaum - - -	0	24	30
	S. Dionyſium - - -	0	27	0
	Plinium - - -	0	29	0
	Poſſidonium - - -	0	31	0
	S. Catharinam - -	0	32	0
	S. Theophil & Cenſorin - -	0	35	30
	Paludem ſomni - -	0	37	30
	Proclum - - -	0	39	30
	Toclenium, & lit. or. Mar. Criſ.	0	40	0
	Lit. Occid. extremum Mar. Criſ.	0	43	0
	Langrenium - - -	0	44	0
	Immerſio totalis propè Nod. Occid.	0	46	30
	Receptio 1ᵃ. Lucis ad Nod. Orient.	3	27	30
	Emergit Grimaldi Margo Orient.	0	30	30
Emergunt ex Umbra	Ejuſdem Margo Occident. - -	0	31	30
	Galilæus - - -	0	32	30
	Ariſtarchus - - -	0	36	0
	Keplerus - - -	0	39	0
	Litus Orient. Maris Humorum	0	39	30
	Gaſſendus - - -	0	43	0
	Plato - - -	0	49	30
	Timocharis - - -	3	51	0
	Tycho totus - - -	0	54	30

		H.	′	″
Emergunt ex Umbra	Sinus æſtuum totus	0	59	0
	Manilius	4	3	0
	Menelaus	0	6	0
	Poſſidonius, & Endymion	0	9	0
	Plinius	0	10	30
	Cenſorinus	0	15	30
	Palus ſomni	0	16	30
	Litus Or. Maris Criſ.	0	19	0
	Litus Occid. extremum	0	22	0
	Langrenius	0	24	30
Finis Eclipſis circà		0	26	0
Nodum Occid.		0	0	0

Horologium correctum per Culminationes Palilicii & aliquot Stellarum Orionis. Diameter Lunæ apparens immediate ante, & poſt Eclipſim dimenſa 32′ 30″ proximè.

V. *Obſervationes aliæ ſelectiores* Ingolſtadii *habitæ Anno* 1726. a Patribus Soc. Jeſu. *Ex eadem Epiſtola.*

	H.	′	″
JAN. 6. Satelles 4 emerſit Teleſc. *Joſephi Campani* ped. 14.	6	40	30
Jan. 19. Mars per vapores tralucens ſtabat ad Lunæ limbum lucidum	6	52	0
Jam erat penitùs immerſus	6	54	0
Centrum Martis emergit è ☾ limbo obſcuro	7	54	25
Totus Mars extra Lunam	7	54	35

Tranſitus

Transitus Martis fuit in lineâ ex centro Grimaldi per extremitatem boream Langreni ductâ. Inde, habitâ ratione librationis Lunaris, collecta centrorum distantia minima, 2′ 30″, Marte australiore. Semidiameter Lunæ apparens hora 9 erat 16′ 55″. Observatio facta Telescopiis 10, ac 12 pedum.

	H.	′	″
Jun. 9. Immers. Intimi; Telescop. 23 ped. - - -	15	4	20
Jul. 17. Immersio ejusdem; dub.	13	24	45
Jul. 20. Immersio 2ⁱ. in 4 Umbr. Tel. 9 ped. - - -	15	16	40
Aug. 1. Evanuit ♂ ex oculis in ☾ limbum obscurum - -	5	25	17
Emersio 1ª. ♂ ad Zoroastrum: Centro suo limbum ☾ lucidum tunc hæc macula occupabat——	6	1	53
Emersio totalis ♂ factâ observatione Telescopiis 12, 14, & 16 ped. - - -	6	1	59
Diameter ☾ apparens hor. 7½, erat 32′ 47″ - - -			
Aug. 2. Immers. Intimi. Telesc. 12 ped.	11	41	20
Aug. 14. Immers. 2ⁱ. eodem Telesc.	12	25	6
Aug. 25. Immers. Intimi. Tel. 23 ped.	11	56	19
Aug. 26. Incipit emergere ex 4 Umbr. Satelles 3ᵘˢ. - -	11	43	17
Sept. 1. Imm. Intimi. Telesc. 23.	13	51	52
Sept. 2. Totalis Immers. Satell. 3ⁱ. in Umbram 4. - -	13	17	32
Eodem die 1ª. Emers. 3ⁱⁱ. è 4 Umbr. Tel. 10. - -	15	45	9
Sept. 9. Immers. 2ⁱ. Telesc. 14.	9	40	circ.

Eodem

	H.	′	″
Eodem die Imm. Intimi. Teleſc. 23.	15	50	30
Eodem die Immerſ. plena Satell^{is}. 3^{ii}. in ♀ Umbr. - -	17	20	30 circ.
Sept. 10. Immerſ. Intimi. - -	10	19	0
Hæ duæ Eclipſes obſervatæ *Biturgi* reſidentiæ *Collegii Ingolſtadienſis*; quam aliàs definivi in ortum vergere 1′ 40″ ab Ingolſt. Meridiano. - -			
Sept. 10. Immerſio Intimi. *Teleſc.* 14.	10	17	10
Sept. 26. Immerſ. ejuſdem, eod. Teleſc.		39	20

Eclipſis Solis ibidem obſervata die 25 *Septembris.*

	H.	′	″
IN loco obſcuro excepta per helioſcopium Solis imago cæptæ Eclipſis initium præbet circa 46° ½ a Nadir ad Boream. - - - -	5	17	22
In ſpeculà aſtroptica 100 circiter paſſibus a loco priori diſtante, Teleſcop. 12, & 16 ped. detegitur Sol jam obſcuratus $\frac{1}{10}$ unius digiti - - -	5	19	24
Immergitur centrum maculæ Solis limbo propinquioris - - -	5	23	30
Centrum maculæ inſignis - -	5	24	40
Centrum maculæ 3^{æ}. - -	5	26	36
2 Digiti obſcurati, a Nadir. in Bor. 39°	5	30	46
3 Digiti - - - - 35 ½	5	37	12
4 Digiti - - - - - 27	5	43	10
Solem 4 Digit. cum dimidio circ. deficientem nubes ſurripuere - -	5	49	

Phaſes

Phaſes Micrometro dimenſæ.

		H.	′	″
Dig. 1	- - - -	5	22	30
2	- - - - -		30	50
3	- - - - -		37	54
4	- - - - -		44	30
4 33′	- - - -		47	30

Solis Semidiameter ſæpius micrometro dimenſa exactè implebat 16′ 0″.

In diſco Solari maculæ à quatuor notatis in immerſione, diverſæ plures apparuere ; Sed eæ exiliores, quàm ut immerſio illarum quoque per vapores Phœbum obſcurantes diſcerni poſſet.

VI. *An Account of a Machine for meaſuring any Depth in the Sea, with great Expedition and certainty ; ſhewn to the Royal Society, by* J. T. Deſaguliers, *L. L. D. and R. S. S. contriv'd by the Rev. Mr.* Stephen Hales, *F. R. S. and Himſelf.*

THere have been ſeveral Machines contriv'd for meaſuring the different Depths of the Sea, eſpecially ſuch as could not be determined by the Lead and Line ; but as thoſe Machines conſiſted of two Bodies (the one ſpecifically lighter, and the other ſpecifically heavier than Water) ſo joined together, that as ſoon as the heavy one came to the Bottom, the lighter ſhould get looſe from it, and emerge ; and the Depth was to be eſtimated by the Time of the Fall of the compound

Body

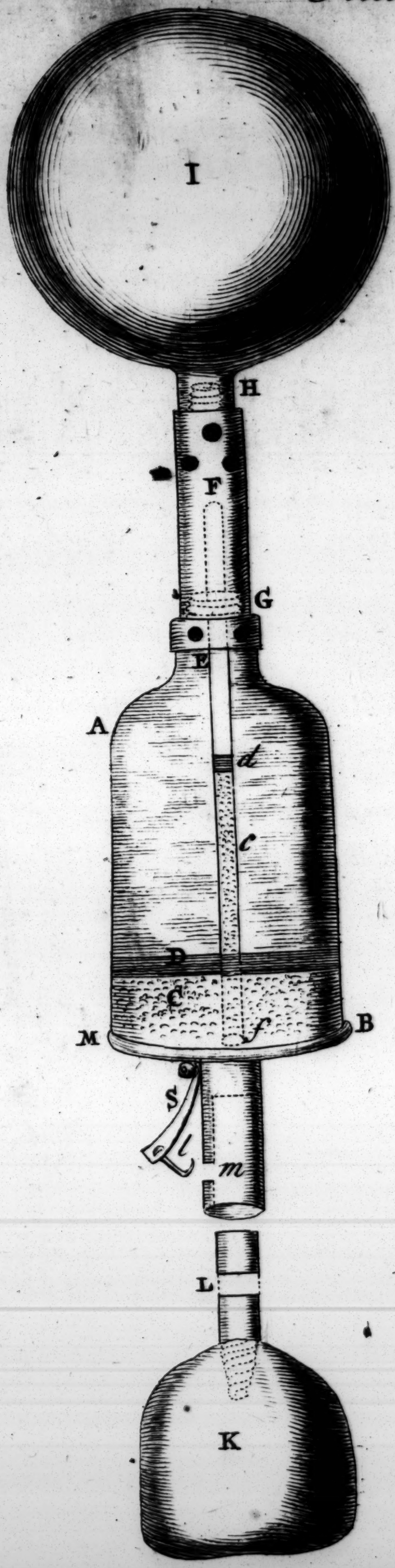
Body from the Top to the Bottom of the Water, together with the Time of the Emerſion of the lighter Body, reckoned from the diſappearing of the Machine, till the emergent Body was ſeen again, no certain Conſequence could be drawn from ſo precarious and complex an Experiment.

For even in ſtill Water, and in the ſame Place, the Time will hardly be the ſame in two Experiments: Much leſs will this Machine anſwer in the Sea, on Account of Waves and Currents, and many other Hindrances.

But as the Preſſure of Fluids in all Directions is always the ſame at the ſame Depth, a Gage which exactly diſcovers what the Preſſure is at the Bottom of the Sea, will ſhew what is the true Depth of the Sea in that Place, whether the Time of the Deſcent of the Machine be but a Minute or two, or twenty Times as long.

The Reverend Mr. *Hales*, in his *Vegetable Staticks*, deſcribes his Gage for eſtimating the Preſſures made in opake Veſſels; where Honey being poured over the Surface of Mercury in an open Veſſel, riſes upon the Surface of the Mercury as it is preſſed up into a Tube whoſe lower Orifice is immerſed into the Honey and Mercury, and whoſe Top is hermetically ſealed. Now as, by the Preſſure, the Air in the Tube is condenſed, and the Mercury riſes, ſo the Mercury comes down again when the Preſſure is taken off, and would leave no Mark of the Height to which it had riſen; but the Honey (or Treacle, which does better) which is upon the Mercury, ſticking to the Inſide of the Tube, leaves a Mark, which ſhews the Height to which the Mercury had riſen, and conſequently makes appear what was the greateſt Preſſure.

My


I
H
F
G
E
A
d
c
D
C
M
f
B
S
l
m
L
K

My Contrivance therefore is a Machine which will carry down Mr. *Hales*'s Gage to the Bottom of the Sea, and immediately bring it up again. *See the Figure.*

A B, is the Gage Bottle.

F f, the Gage Tube cemented to the Braſs Cap of the Bottle at G, with its open End f immers'd in the Mercury C, which by the Preſſure of 32 Foot of Water is carried up to *d* with a little Treacle or Honey *d* upon it, rais'd up from D, a ſmall Thickneſs of Treacle pour'd on upon the Mercury.

When the Preſſure of Water is from a Depth of 64 Foot, the Mercury and Treacle riſe up to E, $\frac{2}{3}$ of the Height of the Tube; and ſo higher proportionably to the Depth.

N. B. *A Scale may be mark'd on the Tube with a Diamond.*

K, is a Weight hanging by its Shank L in a Socket *m*, fix'd to the Ring M B cemented at the Bottom of the Bottle. When the Hole L of the Shank is ſhov'd up to *m*, the Catch *l* of the Spring S holds it from falling out of the Socket, whilſt the Machine is deſcending. But as ſoon as K touches the Ground at the Bottom of the Sea, the Hole L riſing, the Catch flies back and lets go the Weight, as it is ſeen in the Figure. Then the empty Glaſs Ball I (which at Sea may be a Hog's Bladder) riſes up to the Surface of the Water with the Machine, in which obſerving how high the Inſide of the Tube is daub'd, the Preſſure, and conſequently the Depth of the Sea is known.

H G, is a Braſs Tube to guard the Top of the Gage Tube.

There are Holes at F, G and E, to admit the Water to paſs freely every where.

To confirm the Uſe of this Sea-Gage, ſhewn before to the Society, I made another Experiment in the following Manner. Having pour'd ſome Quick-ſilver into the Bottle of the Gage, I pour'd on upon it Treacle to the Depth of half an Inch, then ſcrew'd on the Braſs Cap of the Bottle to which the Glaſs Gage-Tube was cemented; by which Means the open End of the Tube was brought under the Surface of the Mercury, the ſealed End being upwards. The Machine, thus fitted, was immers'd in a cylindrick Veſſel of Water, which with a Plate at Top was preſs'd between two Pillars, in ſuch Manner that Air might be condens'd over the Water without eſcaping. Then having forc'd in ſo much Air with a Syringe, as to lay on a Preſſure equal to what would be in a Depth of 40 Foot of Water, I open'd the Cock of the upper Plate, let out the Air, and, upon taking out the Machine, it appear'd how high the Quick-ſilver had riſen in the Gage-Tube, by the greaſy Mark which the Treacle left within.

VII. *Extract of Two uncommon Caſes of Tumours of the Abdomen, from a Latin Tract publiſh'd at* Strasburgh, Anno 1728, *and entituled,* Joannis Boecleri, M. D. &c. ad Exteros Medicos Epiſtola; *by* W. Rutty, *M. D. R. S. Secr.*

THE firſt is concerning a Woman, an Inhabitant of *Strasburgh*, of Thirty-two Years of Age, whoſe Belly, after an immature and haſty Labour, grew gradually for Ten Years together. During the whole

whole Time of Geſtation, ſhe complain'd of ſcarcely any other Symptom than the Weight and Heavineſs of her Belly; only now and then of a tenſe Pain and a Difficulty in Reſpiration: She ſaid nevertheleſs, that Flatuſes would ſometimes be diſcharged from the *Pudenda*, and the more they were ſo, the leſs Uneaſineſs ſhe perceived. The Menſtrua were regular as to Time; but as to Quantity ſhe did not explain her ſelf: But in the latter Months, towards her Death, ſhe grew plainly cachectick. Her Countenance was cadaverous; her Breaſt and upper Limbs perfectly emaciated; her Feet oedematous, and the Belly much more turgid and prominent than before; ſo that at length ſhe breathed with the utmoſt Difficulty, and upon taking any Nouriſhment, complained of a great Straitneſs in her Cheſt. Upon opening the *Abdomen*, two Days after her Death, ſome Water flow'd out, of a wheyiſh Colour, tho' in what Quantity was not taken Notice of; but upon dividing the *Uterus*, a plentiful Quantity of a bloody Liquor iſſued from it, together with ſeventy-two Molæ of different Figures and Solidity, and chiefly of a black Colour. One only, the 76th, was fix'd to the lower Part of the right Side of the *Uterus*, contiguous to its internal Orifice. Theſe ſolid Subſtances weighed 64 Ounces; as the Liquor alſo filled 15 antient *Alſace* Meaſures, ſo that taken together, the Whole weighed 80 Pounds Apothecaries Weight. The Skin of the *Abdomen* was very thin, and almoſt tranſparent; the Navel perfectly obliterated; the Fat almoſt entirely conſumed; the Muſcles pale, flaccid, and very thin alſo; and the *Peritonæum* in ſome Places ſo ſtrongly attach'd to the *Uterus*, that it could not, without the utmoſt Difficulty, be torn from it. The Body of the

Uterus, which is naturally thick, was extenuated to the ſame Degree of Rarity and Tranſparency with that of the Cutis of the *Abdomen*, and of a ſurpriſing Capacity. The Liver appear'd pale, and ſo flaccid that it might be eaſily rubb'd to Pieces. The Height of the Belly from the Vertebræ of the Loins to the Navel meaſured $1\frac{1}{2}$ Foot; its Length from the *Cartilago enſiformis* to the *Pudenda*, $2\frac{1}{2}$ Feet; and its Circumference at the Waiſt, 4 Feeet $2\frac{1}{2}$ Inches, tho' the Woman was naturally of a ſmall Size and Stature.

The Second is of a Maid-ſervant in the ſame City, of twenty-three Years of Age, whoſe Belly, from a Suppreſſion of the Menſtrua, grew ſlowly for three Years, without any other notable Diſorder; till upon an accidental Fall, it encreaſed ſo much in ſix Days, as to obliterate the Navel; and not being capable of a farther Diſtenſion, Part of the Matter which cauſed the Tumefaction, flowed down to the Legs and ſwelled them likewiſe; which brought on a Difficulty of Breathing, a ſmall, frequent, and uneaſy Pulſe, with a total Loſs of Appetite. But what was more remarkable, the *Syſtole* and *Diaſtole* of the Heart were plainly felt under the left Clavicle, the Heart being, upon Diſſection, found thruſt up to that Part of the *Thorax*. The 14th Day from the Fall, a *Diarrhœa* came on, which kill'd her in a few Days. Upon making a ſmall Inciſion in the right *Hypochondrium*, there guſhed out from the Cavity a Liquor, in Colour, Conſiſtence, and Froth reſembling well boiled Beer; which upon enlarging the Inciſion, was followed by a fœtid purulent Matter, with entire Portions of the putrified Caul; which Matter filled 56 *Strasburgh* Pints. Upon this the Belly ſubſided; but a large ſolid Subſtance ſtill remained

mained under the containing Parts of the *Abdomen*. Opening therefore the whole Cavity, there was found under the left Groin a conſiderable Tumour, nouriſhed by its proper Veſſels, and every where fixed to the circumjacent Membranes; which being freed from, it weighed 6 Pounds common Weight. This Tumour proved a Congeries of incyſtated Abceſſes (wrapt up in one common Covering) of different Sizes; the largeſt as big as a Man's two Fiſts, the ſmalleſt the Size of an Egg; and each of a different Sort of Subſtance: Beſides which was a great Number of *Hydatides*. The *Peritonæum* was as thick as the *Cutis*; the Caul almoſt entirely deſtroyed; the Stomach natural, but perfectly empty; the Guts livid, very much thickened and vaſtly inflated, and moreover connected preternaturally to each other by peculiar Membranes. The Liver ſtrongly adhered to the right *Hypochondrium*, and its Coat parted from its *Parenchyma* almoſt ſpontaneouſly. The left Kidney very near equal'd the Spleen in Bulk, and the *Pancreas* was as hard as a Cartilage; but the *Uterus* and Bladder were found in *ſtatu ſano*. The Cavity of the *Thorax* was much ſmaller than uſual, from the Contents of the *Abdomen* preſſing up the *Diaphragm* into it; in which Cavity alſo was found the ſame Sort of bloody putrid Liquor, as likewiſe in the *Pericardium*. The right Ventricle of the Heart was preternaturally ſoft and flaccid, and being opened, was lined with Hydatides. The upper Parts of the Body were emaciated; the lower much tumefied by the Water contained within them.

The Author, after this, cites three other Caſes of extraordinary preternatural Tumours of the *Abdomen*, communicated to him by Dr. *Valentine Scheide*, the

preſent

preſent chief Phyſician of *Strasburgh*, all which occurr'd in his own Practice; as the foregoing are recorded in the Regiſter of that Univerſity. Theſe likewiſe are very remarkable: But as they are not altogether ſo uncommon, a particular Account need not be given of them.

VIII. *An Account of the Culture and Management of Saffron in* England, *by* James Douglaſs, M. D. Med. Regin. Extr. & S. R. S.

AS Saffron grows at preſent moſt plentifully in *Cambridge-ſhire*, and has grown formerly in ſeveral other Counties of *England*, the Method of Culture does not, I believe, vary much in any of them, and therefore I have judged it ſufficient to ſet down here the Obſervations which I employed proper Perſons, in different Seaſons, to make in the Years 1723, 24, 25, and 28, up and down all that large Tract of Ground that lies between *Saffron-Walden* and *Cambridge*, in a Circle of about ten Miles Diameter. In that Country Saffron has been longeſt cultivated, and therefore it may reaſonably be expected that the Inhabitants thereof are more throughly acquainted with it than they are any where elſe.

I ſhall begin with the Choice and Preparation of the Ground. The greateſt Part of the Tract already mention'd is an open level Country with few Incloſures; and the Cuſtom there is, as in moſt other Places, to crop two Years, and let the Land lie fallow the third. Saf-

fron

fron is always planted upon fallow Ground, and all other Things being alike, they prefer that which has born Barley the Year before.

The Saffron-grounds are ſeldom above three Acres, or leſs than one, and in chooſing them, the principal Thing they have Regard to is, that they be well expoſed, the Soil not poor, nor a very ſtiff Clay, but a temperate dry Mold, ſuch as commonly lies upon Chalk, and is of a hazel Colour; though if every Thing elſe anſwers, the Colour of the Mold is pretty much neglected.

The Ground being made choice of, about Lady-day, or the Beginning of *April*, it muſt be carefully ploughed, the Furrows being drawn much cloſer together and deeper, if the Soil will allow it, than is done for any Kind of Corn, and accordingly the Charge is greater.

About five Weeks after, or during any Time in the Month of *May*, they lay between twenty and thirty Loads of Dung upon each Acre, and having ſpread it with great Care, they plough it in as before. The ſhorteſt rotten Dung is the beſt; and the Farmers who have the Conveniencies of making it, ſpare no Pains to make it good, being ſure of a proportionable Price for it. About Midſummer, they plough a third Time, and between every ſixteen Foot and an half, or Pole in Breadth, they leave a broad Furrow or Trench, which ſerves both for a Boundary to the ſeveral Parcels, (when there are ſeveral Proprietors to one Encloſure) and to throw the Weeds in at the proper Seaſon.

To this Head likewiſe belongs the Fencing of the Grounds, becauſe moſt commonly, though not always, that is done before they plant. The Fences conſiſt of what they call dead Hedges, or Hurdles to keep out not only

only Cattle of all Sorts, but eſpecially Hares, which would otherwiſe feed on the Saffron Leaves during the Winter.

About the Weather we need only obſerve, that the hotteſt Summers are certainly the beſt, and if therewith there be gentle Showers from time to time, they can hardly miſs of a plentiful rich Crop, if the extream Cold, Snow, or Rain of the foregoing Winter have not prejudiced the Heads.

The next general Part of the Culture of Saffron, is planting or ſetting the Roots; the only Inſtrument uſed for which, is a narrow Spade, commonly termed a *Spit-ſhovel.*

The Time of Planting is commonly in the Month of *July*, a little ſooner or later, according as the Weather anſwers. The Method is this. One Man with his Spit-ſhovel raiſes between three and four Inches of Earth, and throws it before him about ſix, or more Inches; two Perſons, generally Women, following him with Heads, place them in the fartheſt Edge of the Trench he makes at three Inches diſtance from one another, or thereabouts. As ſoon as the Digger or Spitter has gone once the Breadth of the Ridge, he begins again at the other Side, and digging as before, covers the Roots laſt ſet, and makes the ſame Room for the Setters to place a new Row, at the ſame Diſtance from the firſt, that they are from one another. Thus they go on till a whole Ridge, containing commonly one Rod, is planted, and the only Nicety in digging is to leave ſome Part of the firſt *Stratum* of Earth untouched to lie under the Roots; and in ſetting, to place the Roots directly upon their Bottoms. What Sort of Roots are to be preferred, ſhall be ſhown under the fourth Head;

but

but it muſt be obſerved in this Place, that formerly when Roots were very dear, they did not plant them ſo thick as they do now; and that they have always ſome Regard to the Size of the Roots, placing the largeſt at a greater Diſtance than the ſmall ones.

The Quantity of Roots planted in an Acre is generally about ſixteen Quarters, or 128 Buſhels, which according to the Diſtances left between them, as before aſſigned, and ſuppoſing them all to be an Inch in Diameter one with another, ought to amount to 392040 in Number.

From the Time that the Roots are planted, till about the Beginning of *September*, or ſometimes later, there is no more Labour about them; but as they then begin to ſpire, and are ready to ſhew themſelves above Ground, which is known by digging a few out of the Earth, the Ground muſt be carefully pared with a ſharp Hough, and the Weeds, *&c.* raked into the Furrows, becauſe otherwiſe they would hinder the Growth of the Plants.

In ſome Time after appear the Saffron Flowers, and this leads us to the third Branch of our preſent Method. The Flowers are gathered as well before, as after they are full blown, and the moſt proper Time for this, is early in the Morning. The Owners of the Saffron get together a ſufficient Number of Hands, who place themſelves in different Parts of the Field, pull off the whole Flowers, and throw them Handful by Handful into a Basket; and ſo continue till all the Flowers are gathered, which happens commonly about ten or eleven o'Clock.

Having then carried home all they have got, they immediately ſpread them upon a large Table, and placing themſelves round it, they fall to picking out the Filamenta Styli, or Chives, and together with them, a

pretty long Portion of the Stylus itſelf, or *String* to which they are joined. The reſt of the Flowers they throw away as uſeleſs. The next Morning they return into the Field again, whether it be wet or dry Weather, and ſo on daily, even on Sundays, till the whole Crop be gathered.

The Chives being all picked out of the Flowers, the next Labour about them is to dry them on the Kiln. The Kiln is built upon a thick Plank (that it may be moveable from Place to Place) ſupported by four ſhort Legs. The Outſide conſiſts of eight Pieces of Wood, about three Inches thick, joined in Form of a quadrangular Frame, about twelve Inches ſquare at Bottom on the Inſide, and twenty-two Inches at Top, which is likewiſe equal to the perpendicular Height of it. On the Foreſide is left a Hole about eight Inches ſquare, and four Inches above the Plank, through which the Fire is put in. Over all the reſt, Laths are laid pretty cloſe to one another, and nailed to the Frame already mentioned, and then are plaiſtered over on both Sides, as is alſo the Plank at Bottom very thick, to ſerve for a Hearth. Over the Mouth, or wideſt Part, goes a Hair-Cloth fixed to two Sides of the Kiln, and likewiſe to two Rollers, or moveable Pieces of Wood, which are turned by Wedges or Screws, in order to ſtretch the Cloth. Inſtead of the Hair-Cloth many People now uſe a Net-work of Iron-wire, with which it is obſerved, that the Saffron dries ſooner, and with a leſs Quantity of Fewel; but the Difficulty of preſerving the Saffron from burning, makes the Hair-Cloth be preferred by the niceſt Judges in drying.

The Kiln is placed in a light part of the Houſe, and they begin by laying five or ſix Sheets of white Paper on the Hair-cloth, upon which they ſpread the wet Saffron, between two and three Inches thick.

This

This they cover with other Sheets of Paper, and over theſe lay a coarſe Blanket five or ſix times doubled, or inſtead thereof, a Canvas Pillow fill'd with Straw, and after the Fire has been lighted for ſome time, the whole is cover'd with a Board, having a large Weight upon it.

At firſt they give it a pretty ſtrong Heat, to make the Chives ſweat, as their Expreſſion is; and in this, if they do not uſe a great deal of Care, they are in danger of ſcorching, and ſo of ſpoiling all that is on the Kiln.

When it has been thus dry'd for about an Hour, they take off the Board, Blanket, and upper Papers, and take the Saffron off from that that lies next it, raiſing at the ſame time the Edges of the Cake with a Knife. Then laying on the Papers again, they ſlide in another Board between the Hair-Cloth and under-Papers, and turn both Papers and Saffron upſide down, afterwards covering them as above.

This ſame Heat is continued for an Hour longer; then they look to the Cake again, free it from the Papers and turn it; then they cover it, and lay on the Weight as before. If nothing happens amiſs, during theſe firſt two Hours, they reckon the Danger to be over; for they have nothing more to do, but to keep a gentle Fire, and turn their Cake every half Hour, 'till it be thoroughly dry; for doing which as it ought, there are required full twenty-four Hours.

In drying the large plump Chives they uſe nothing; but towards the latter End of the Crop, when theſe come to be ſmaller, they ſprinkle the Cake with a little ſmall Beer, to make it ſweat as it ought; and they begin now to think, that uſing two linnen Cloths next the Cake, inſtead of the two innermoſt Papers, may be of ſome Advantage in drying; but this Practice is followed as yet but by few.

Their Fire may be made of any kind of Fewel;

but that which ſmoaks the leaſt is beſt, and Charcoal for that Reaſon is preferred to any other.

What Quantity of Saffron a firſt Crop will produce is very uncertain. Sometimes five or ſix Pounds of wet Chives are got from one Rood; ſometimes not above one or two, and ſometimes not enough to make it worth while to gather and dry it. But this is always to be obſerved, that about five Pounds of wet Saffron go to make one Pound of dry, for the firſt three Weeks of the Crop, and ſix Pounds during the laſt Week; and now the Heads are planted very thick, two Pounds of dry'd Saffron may, at a Medium, be allow'd to an Acre for a firſt Crop, and four and twenty Pounds fo t he two remaining, the third being conſiderably larger than the ſecond.

In order to obtain theſe, there is only a Repetition to be made every Year of the Labour of houghing, gathering, picking and drying in the ſame manner as before ſet down, without the Addition of any thing new; except that they let Cattle into the Fields, after the Leaves are decay'd, to feed upon the Weeds; or perhaps mow them for the ſame Uſe.

About the *Midſummer* after the third Crop is gathered, the Roots muſt all be taken up and tranſplanted: The Management requiſite for which is the fourth Thing to be treated of. To take up the Saffron Heads, or break up the Ground, as their Term is, they ſometimes plough it, ſometimes uſe a forked Kind of Hough called a Pattock, and then the Ground is harrowed once or twice over; during all which Time of ploughing, or digging and harrowing, fifteen or more People will find Work enough to follow and gather the Heads as they are turned up.

They are next to be carried to the Houſe in Sacks, and there to be clean'd or raſed. This Labour conſiſts in clearing the Roots thoroughly from Earth, and

from the Remains of old Roots, old Involucra, and Excreſcencies; and thus they become fit to be planted in new Ground immediately, or to be kept for ſome Time without Danger of ſpoiling.

The Quantity of Roots taken up, in Proportion to thoſe that were planted, is uncertain; but at a Medium it may be ſaid, that allowing for all the Accidents that happen to them in the Ground, and in breaking up, from each Acre may be had twenty-four Quarters of clean Roots, all fit to be re-planted. The Owners are ſure to chooſe for their own Uſe the largeſt, plumpeſt, and fatteſt Roots, but above all, they reject the longiſh pointed ones, which they call *Spickets* or *Spickards*; for very ſmall round or flat Roots are ſometimes obſerved to flower.

This is the whole Culture of Saffron in the Country above-mentioned; and we have only now to conſider the Charges and Profits which may be ſuppoſed, one Year with another, to attend this Branch of Agriculture; and of theſe I have drawn up the following Computation for one Acre of Ground, according to the Price of Labour in this County.

	l.	*s.*	*d.*
Rent for three Years	3	0	0
Ploughing three Times	0	18	0
Dunging	3	12	0
Hedging	1	16	0
Spitting and ſetting the Heads	1	12	0
Weeding, or paring the Ground	1	4	0
Gathering and picking the Flowers	6	10	0
Drying the Flowers	1	6	0
Inſtruments of Labour for three Years with the Kiln, about	0	10	0
Ploughing the Ground once and harrowing twice	0	12	0
Gathering the Saffron Heads	1	0	0
Raſing the Heads	1	12	0
Total Charge	23	12	0

This Calculation is made upon the Suppoſition, that an Acre of Ground yields twenty ſix Pounds of neat Saffron in three Years, which I ſtated only as a mean Quantity between the greateſt and the leaſt; and therefore the Price of Saffron muſt be adjuſted accordingly, which I think cannot be done better than by fixing it at thirty Shillings per Pound; ſince in very plentiful Years it is ſold for twenty, and is ſometimes worth between three and four Pounds. At this Rate, twenty-ſix Pounds of Saffron are worth thirty-nine Pounds, and the neat Profits of an Acre of Ground producing Saffron, will in three Years amount to fifteen Pounds thirteen Shillings, or to about five Pounds four Shillings yearly. This, I ſay, may be reckoned the neat Profit of an Acre of Saffron, ſuppoſing that all the Labour were to be hired for ready Money; but as the Planter and his Family do a conſiderable Part of the Work themſelves, ſome of this Expence is ſaved: That is, by planting Saffron, he not only may reaſonably expect to clear about five Pounds yearly *per* Acre, but alſo to maintain himſelf and Family for ſome Part of each Year; and it is upon this Suppoſition only, that the Reſult of other Computations which have been made of the Profits of Saffron, can be ſaid to have any tolerable Degree of Exactneſs; but the Calculations themſelves are undoubtedly very unaccurate.

I have ſaid nothing here concerning the Charge in buying, or Profits in ſelling the Saffron Heads, becauſe in any large Tract of Ground theſe muſt at length always ballance one another, while the Quantity of Ground planted yearly continues the ſame, which has been pretty much the Caſe for ſeveral Years paſt.

F I N I S.

Jos: Banks

Numb. 406.

PHILOSOPHICAL TRANSACTIONS

FOR THE

Month of *DECEMBER*, 1728.

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I. Re-

I. *Remarks on the Height of Mountains in general, and of thoſe of* Swiſſerland *in particular, with an Account of the Riſe of ſome of the moſt conſiderable Rivers of* Europe. *By* J. G. Scheuchzer, *M. D. &c.*

IN a former Paper, I took Notice that *Dicæarchus* found *Mount Pelius* in *Theſſalia*, to be 1250 Paces high, which make 6250 *Roman*, or 6822 *Paris* Feet, a Height which we may well pronounce too great even for the abſolute Height of *Mount Pelius*, I mean its Riſe above the Level of the Sea. Conform to the Determination of *Dicæarchus*, I mentioned, that *Plutarch* fixes the Height of the higheſt Mountains, and the greateſt Depth of the Sea to 10 Stadia, and *Cleomedes* affirms, that they cannot exceed 16 Stadia. The celebrated *Galileus de Galileis* is one of the moſt modeſt among the modern Writers on this Head: For he ſays, * that the higheſt Mountains do not riſe above a Mile, or 8 Stadia, or 5000 old *Roman Veſpaſian* Feet, which make 5458 *Paris* Feet above the Level of the Sea, which we ſhall find by and by to agree pretty well with ſome of the higheſt Mountains in *France*, and may conjecture to do ſo with thoſe in *Italy*. *Kepler* went rather too far † when he aſſigned the Mountains of *Rhætia* (thought the higheſt in *Swiſſerland*) a Height of 26 Stadia, or 10000 old *Roman Veſpaſian* Feet, which make 10916 *Paris* Feet. The Opinions of ſome other antient and modern Geographers and Mathematicians, will appear better by the Table annexed.

* Nuntius Sidereus, p. 14.

† Aſtronom. Optic. p. 129, 135. & Epitom. Aſtronom. lib I. pag. 26.

A Table ſhewing the Height of Mountains according to ſeveral antient and modern Writers.

	Stadia.	Old *Roman Veſpaſian* Feet.	*Paris* Feet.
Strabo (*Lib.* II. *Geog.*) ſays, that the higheſt Mountain, called by him *Petra Sogdiana*, is of - - - -	30	18750	20468
Pererius (*Lib.* XII. *in Geneſin*) determines the higheſt Mountains to - - -	32	20000	21832
Leo Bapt. Albertus (*Architect. Lib.* x. *Cap.* i.) to - -		22500	23661
Ath. Kircher. (*Ars magn. luc. & umbr. P.* II. *Probl.* 5.) brings them to - -	43	26875	29337
Fromond. (*Lib.* I. *Meteor. Cap.* 2. *Art.* i. - -	64	40000	43664
Gilbertus de magnete. L. IV. *C.* i.	128	80000	87328
Pliny (*Lib.* III. *Cap.* lxiv.) according to the Explanation of *Fortunius Licetus* (*de Lunæ Luce ſubobſcura, Lib.* II. *p.* 306.) to - - -	400	250000	272900
Ricciolus, Geophr. (*Lib.* VI.) is of Opinion, in Purſuance of what he imagines to have demonſtrated of the Mountains *Athos* and *Caucaſus*, that poſſibly there may be Mountains of - - - -	512	320000	349312

Now,

Now, in Oppoſition to this Table, wherein the Heights muſt needs, upon firſt View, appear romantick and unnatural, let us conſider the Height of ſuch Mountains, as have been meaſured, either by Trigonometrical or Barometrical Obſervations.

In *England*, the Height of *Snowdon-hill*, one of the higheſt Mountains in *Wales*, was meaſured Trigonometrically, by Mr. *J. Caſwell* of *Oxford*, and found to be of 1240 Yards, or 3720 *Engliſh* Feet, which make 3488 *Paris* Feet. At the Top of this Mountain, the Mercury ſubſided to 25′′ 6′′′, which being reduced to *Paris* Meaſure, make juſt 24′′. Now in the Tables above, the Height of the Place where the Mercury ſubſides to 24′′, is, according to *Mariotte*, of 544 Toiſes, two Foot, or 3266 Foot above the Level of the Sea, according to *Caſſini*, 676 Toiſes, or 4056 Feet, and according to my Uncle's Calculation 559° 2′, or 3356′, ſo that *Mariotte* comes 222 Feet ſhort of its Height, as it was determined Trigonometrically, Dr. *Scheuchzer* but 132′, but *Caſſini* exceeds this Height by 568 Feet, which confirms again, as I have ſhewn in a former Paper, that the *Mariottian* Table is preferable to that of *Caſſini*, though pretended to have been corrected upon the former, and that that of Dr. *Scheuchzer* is an Improvement upon both. According to the Obſervation made by Dr. *Halley*, *May* 26, 1697, the Mercury ſtood at the Top of *Snowden-hill*, at 26′′ 1′′′ *Engliſh*, which, if reduced as above, would give the Height of the Mountain ſomething leſs.

In *France*, when the Meridian Line, firſt begun in 1669, was continued in 1703, the Heights of ſeveral Mountains, particularly in the South of *France*, were determined Trigonometrically by the Members of the

Royal Academy of Sciences: And I find up and down in their Memoirs, the Heights of the following.

	Height in Toiſes.	Feet.
Mont Clairet in Provence -	277 or	1662
La Maſſane in Rouſſilion - -	397 —	2382
The ſame according to another Obſervation - - - - -	408 —	2448
Bugarach, a Mountain in *Languedoc* - - - - -	648 —	3888
Mountains in *Auvergne.*		
Le Puy de Domme, near *Clermont*	810 —	4860
La Courlande - -	838 —	5028
La Coſte - - -	851 —	5106
Le Puy de Violent - -	853 —	5118
Le Cantal - - -	984 —	5904
Le Mont d' or - - -	1030 —	6180
In the County of *Avignon.*		
Le Mont ventoux - -	1036 —	6216
Pyrenean Mountains.		
S. Barthelemy dans le paix de foix - - -	1185 —	7110
La Montagne du Mouſſet -	1258 —	7548
Le Canigou - - -	1440 —	8640

Before I proceed farther, I muſt beg Leave to obſerve, that the Heights of theſe Mountains, in the main, ſeem rather too great. This indeed is eaſily accounted for, as they were meaſured by Trigonometrical Obſervations, which will, as I have took Notice above, becauſe of the Re-

Refraction of the Air, give the Heights greater than they actually are. But what confirms it ſtill more, is, that according to the Tables above, the Numbers which anſwer to the Heights of the Mercury, as they were obſerved at the Top of ſome of thoſe Mountains, are conſiderably leſs, and that even Monſ. *Caſſini*'s own Numbers, which yet we have by ſome undoubted Experiments ſhewn to be too great, fall often ſhort. It will be enough to mention two or three Inſtances. At the Tower of *Maſſane* in *Rouſſillon*, the Mercury ſtood at 25'' 5''', and the Height of that Place was determined trigonometrically, of - - 397 Toiſes.

Now 25'' 5''' anſwer according to *Mariotte*, to - -	342	0
According to *Caſſini*, - -	392	4
According to Dr. *Scheuchzer* - -	350	0

At the Top of the Mountain called *la Coſte* in *Auvergne*, the Mercury ſtood, *Oct.* 9, 1700, at 23''' 4''', and the Height of this Mountain was determined Trigonometrically of - - 851° Toiſes.

Now 23'' 4''' anſwer according to *Mariotte*, to	644°	1'	differ.	206° 5'
Caſſini - -	826	1		24 5
Dr. *Scheuchzer* - -	661	5		189 1

The Difference is ſtill more conſiderable with Regard to the high Mountain *Mont d'or* en *Auvergne*, the Height whereof was determined Trigonometrically to - - 1040 Toiſes.

At

At the Top of this Mountain the Mercury fell, according to an Obſervation made by *F. Sebaſtien Truchet, June* 8, 1705, to 22′′ 11′′′, which anſwer according to

Mariotte, to	707°	5′	differ.	3328	1′
Caſſini to	925	1		114	5
Dr. *Scheuchzer*	727	3		312	3

I come now to the Mountains of *Swiſſerland*. The Barometrical Obſervations made by my Father upon ſeveral of the higheſt will convince us, that they riſe aloft, above all the neighbouring ones in *France*, *Spain*, *Italy* and *Germany*. And that it muſt be ſo appears farther, becauſe from their elevated Tops, they diſpenſe their Waters to all the *European* Kingdoms and Provinces around them. Nay, I doubt not, but that they may vye in Height with the moſt conſiderable Mountains in any other Part of the known Globe. *Swiſſerland* it ſelf, I mean its Valleys and lower Parts, as they are conſiderably remote from the Sea, riſe alſo in Proportion above the Level of it. 'Tis true, the Aſcent thither is but gradual, in Proportion to the Remoteneſs. At *Zuric*, for Inſtance, which lies towards the Northern Borders of *Swiſſerland*, the mean Height of the Barometer hath been obſerved of 26′′ 5′′′, which give the Elevation of that Town, above the Level of the Sea, according to *Mariotte*, 205 Toiſes, 4 Foot, or 1234′, according to Dr. *Scheuchzer*, 210° 4′, or 1264′, and according to *Caſſini*, 221° 4′, or 1330′. This Town is diſtant from the Mouth of the Rhine, which is the neareſt Part of the Ocean, at leaſt 375 *Engliſh* Miles, or an hundred marine *French* Leagues, and from *Genoa* which is neareſt upon the Mediterranean, 225 *Engliſh*

gliſh Miles, or 62 *French* marine Leagues. So that going down from *Zuric* Northwards towards the Sea, the Deſcent, or Fall, is but ſomething more than 12 Foot, for a marine League of *France*, if we ſuppoſe a ſtreight Line to be drawn from *Zuric* to the Sea-ſhore in *Holland*; but it is much greater going Southward towards the Mediterranean, where it comes at leaſt to 20 Foot for one League.* Nay, if we conſider that the higheſt Mountains of *Swiſſerland* lie almoſt directly between *Zuric* and the Mediterranean Shores, we muſt allow ſo much more in Proportion, as thoſe Mountains are elevated above the Horizon of *Zuric*, and how great and ſudden this Elevation be, will appear by the following Obſervations.

At *Ennen Sewen gen Aweren* in the Aſcent of the high Mountain *Freyberg*, in the Canton of *Glarus*, which lies South Eaſt of *Zuric*, the Mercury was obſerved *Sept.* 11, 1710, at 23″ 10‴, which gives the Height of that Place above the Level of the Sea, according to

Mariotte - -	569°	2′	or 3416′
Dr. *Scheuchzer* -	584	4	— 3508
Caſſini - - -	712	3	— 4275

Upon *Scherf*, one of the Branches of the *Freyberg*, the Mercury fell *Sept.* 12, 1710, to 21″ 8‴, which gives the Height of that Part of the Mountain according to

Mariotte - -	906°	1′	or 5437′
Dr. *Scheuchzer* - -	931	2	or 5588
Caſſini - - -	1247	4	or 7486

Still

Still higher upon *Blattenſtock*, another Part of the ſame Mountain, the Mercury fell on the ſame Day to 21″ 6‴, which anſwer according to

Mariotte, to	- -	933° 2′	or 5600′
Dr. *Scheuchzer*	- -	959 2	or 5756
Caſſini	- -	1293 3	or 7761

Hence from *Zuric* to the *Blattenſtock* near the Top of the *Freyberg*, there is, in leſs than three Days Journey, a Riſe of 4366 Feet, according to *Mariotte*, and 4492, according to Dr. *Scheuchzer*, that is, more than three times the Elevation of *Zuric* above the Level of the Sea.

At *Guppen ob Schwanden*, in the ſame Canton of *Glarus*, the Mercury was obſerved, *Auguſt* 5, 1705, at 23″ 4‴, which give, according to

Mariotte	- -	644° 1′	or 3865′
Dr. *Scheuchzer*	- -	661 5	or 3971

(I omit giving the Numbers according to the Tables of Mr. *Caſſini*, having already ſhewn, that they are too great) The Height of this Mountain is nearly the ſame with the celebrated *Puy de Domme*, where Monſ. *Perier* obſerved the Mercury, *Sept.* 19, 1648, at 23″ 2‴.

Upon *Joch*, a high Mountain in the Territory of *Engelberg*, where it confines upon the Canton of *Bern*, full South of *Zuric*, the Mercury ſtood, *June* 23, 1706, at 21″ 4‴, which gives the Height of that Mountain according to

Mariotte	- -	961° 0′	or 5766
Dr. *Sheuchzer*,	- -	987 4	or 5926

This Mountain, though very high, is far from being the higheſt in that Neighbourhood, for next to it there

there riſes another called the *Titlisberg*, covered with everlaſting Snow, which we may, upon a moderate Computation, pronounce at leaſt 1000 Foot higher than the Top of the *Joch*, and conſequently one of the higheſt in the Country.

Upon the *Avicula*, by the *Italians* called *Monte del' Uccello*, and by ſome S. *Bernhard*'s Mountain, from a Chappel built in Honour of that Saint, a high Mountain in *Rhætia*, towards *Italy*, the Mercury was obſerved, *July* 30, 1707, at 22″ 11‴, which give according to

Mariotte	-	-	707°	5′	or 4247′
Dr. *Scheuchzer*	-	-	727	3	4365

This Height muſt be underſtood only of that Part of the Mountain which is paſſed over by Travellers, the Mountain it ſelf riſing conſiderably above it, and the *Adula*, or Διαδυέλλας of *Strabo*, Geog. L. III. of which the *Avicula* is only a Part, being ſtill higher. The *Rhenus poſterior*, or *Hinder Rhein*, and the *Moüſs*, which at laſt loſes itſelf into the *Teſin*, near *Bellinzone*, not much above the Entry of the *Teſin* into the Lake of *Locarno*, ariſe upon this Mountain.

At *Santa Maria*, upon the *Luckmannier Berg*, by ſome S. *Barnaby*'s Mountain, which is likewiſe a Branch of the *Adula*, the Mercury ſtood, *Aug.* 9, 1725, as upon the *Avicula*, at 22″ 11‴, which ſhews the Height of theſe two Places to be equal.

In the Alp *San Porta*, near the Source of the *Hinter Rhein*, *Rhenus poſterior*, five Hours and a half from *Speluga*, *Splügen* in *Rhætia*, the Mercury was obſerved, *July* 29, 1707, at 21″ 4‴, where it ſtood likewiſe upon the above-mentioned Mountain *Joch*, whither the Reader is referred for the Height of this Alp.

At *Splügen* itſelf, the Mercury ſtood the ſame Morning early, at 23″ 4‴, which give the Elevation of *Splügen* according to *Mariotte* 644° 1′ or 3865, and according to Dr. *Scheuchzer*, 661° 5′ or 3971′. So that the Fall of the Rhine from the Alp aforeſaid to *Splügen*, in five Hours and a half, comes, according to *Mariotte*, to 1901, and according to Dr. *Scheuchzer*, to 1955 *Paris* Feet perpendicular.

At the *Capuchins*, upon the high Mountain S. *Gothard*, a celebrated Paſſage out of *Swiſſerland* into *Italy*, the Mercury ſtood, *June* 30, 1705, at 22″ 0, which gives the Height of that Paſſage, which with Regard to the higheſt Tops of S. *Gothard*, lies but as it were at the Foot of a high Mountain, according to *Mariotte* 852°, or 5112′, and according to Dr. *Scheuchzer*, 875° 5′, or 5255, above the Level of the Sea.

Upon the *Furca*, a high Mountain between the *Urſeren Thal*, *Urſaria Vallis*, and the upper *Valleſia*, and one of the Branches of the S. *Gothard*, the Height of the Mercury in the Barometer was obſerved, *July*, 31, 1707, at 21″ 5‴, which give the Height of this Mountain above the Level of the Sea, according to *Mariotte*, 947° 1′ or 5683′, and according to Dr. *Scheuchzer*, 973° 3′ or 5841. Near this Mountain there are others, which cannot be leſs than 800 or 900 Foot higher.

Theſe Mountains, I mean the *Avicula*, the *Luckmannier Berg*, the S. *Gothard*, and the *Furca*, together with the *Grimſula*, the *Criſpalt*, the *Sempronier*, or *Sempronius Mons*, the *Adula*, and a Chain of others, are the *Lepontiæ Alpes* of *Pliny* * and the *Summæ*

* Lib. iii. c. xx.

Alpes

Alpes of *Cæſar* *. They begin in the upper *Valleſia*, traverſe the Canton of *Uri*, and ſo run on Eaſtwards, a-croſs the Country of the *Griſoons*, towards *Tirol*. Their greateſt Height above the Level of the Sea, may be fixed in round Numbers to 7500, or 8000 *Paris* Feet.

'Tis upon theſe very Mountains, that ſome of the moſt conſiderable Rivers of *Europe* take their firſt Riſe, within very ſmall Diſtances of each other. The *Rhoſne*, for Inſtance, *Rhodanus*, by *Marcellinus* called, *maximi nominis flumen*, and by *Varro*, *Fluvius inter tres Europæ maximus*, ariſes from two *Gletchers*, as we call them, or *Montes glaciales*, *huge Mountains of Ice*, near the *Furca*, whoſe Height hath been above determined, and thence runs with great Impetuoſity down *Valleſia*, the *Walliſſerland*, forming a long Valley, ſurrounded on both Sides with huge Mountains, till it looſes its Waters and Name in the *Lacus Lemannus*, or *Lake* of *Geneva*, but reſumes it again near the Town of *Geneva*, whence it flows with a more gentle Deſcent through ſome Provinces of *France* into the Mediterranean Sea.

The *Theſin*, *Ticinus*, by *Claudian*, in his Panegyric upon the Conſulate of the Emperor *Honorius*, called *Pulcher*, *the handſom*, takes its firſt Riſe from two ſmall Lakes upon the S. *Gothard*, and ſome lateral Sources from the *Lago ſopra la Cima di Pettine*, upon a Mountain called *Pettine*, the *Lago della Sella*, the Lake of *Rottom* upon the *Luckmannier Berg*, the Lake of *Tom*, and the *Lake of Bedretto*, upon a Mountain of this Name. It deſcends the *Lavinia Vallis*, or *Liviner Valley*, and in its Way to the Lake of

* De Bello Gallico l. iii.

Locarno, receives many Brooks and Rivulets from the adjoining Mountains: It unites its Waters with the *Po*, near *Pavia*, and looſes itſelf jointly with that River into the Adriatick Gulf.

The Rhine, *Rhenus*, by *Cæſar de Bello Gallico* termed, *latiſſimus atque altiſſimus*, ariſes in three ſeveral Branches, which are called *Rhenus anterior, poſterior, & medius*, the *further*, the *hinder*, and *middle Rhine*. The *hinder Rhine* takes its Riſe upon the high Mountain *Avicula, Colmen del Occello*, Part of the *Adula*, in the Alp *San Porta*, from a *Gletcher*, or Ice-mountain, which extends in Length full two Hours. The *middle Rhine*, *Rhenus medius*, ariſes upon the *Luckmannier Berg*, which is likewiſe Part of the *Adula*, in the upper Part of a Valley called *San Maria*, oppoſite to one of the Sources of the *Theſin*. The *furthermoſt Rhine*, *Rhenus anterior*, ariſes upon that Branch of the *Criſpalt*, which is called *Cima del Badut*, *Badùz*, and ſoon receives ſeveral lateral Branches from the *Alps Mugels* and *Cornera*. My preſent Purpoſe will not ſuffer me to purſue the Courſe of this River in its ſeveral Branches. Near the Monaſtery of *Diſentis*, the further and middle Rhine join together, and the united Stream falls into the hinder Rhine, near *Reichenau*. Below *Rheineck*, the Rhine falls into the *Lacus Bodamicus*, or *Boden Sea*, and comes out of it near *Stein*; whence waſhing for ſometime the Borders of *Swiſſerland*, it then traverſes great Part of *Germany* in a very irregular Courſe, till at laſt, in *Holland*, it looſes itſelf into the great Ocean.

The *Reüſs*, *Ruſa*, ariſes from a ſmall Lake called *Lago di Luzendro*, upon the S. *Gothard*, but ſoon receives a conſiderable Inforcement from the *Furca*,

and

and near *Urſelen*, another from a mountainous Lake in *Oberalp*. Near *Flüelen*, not far from *Ury*, it enters the IV. *Waldſtetten Sea*, *Lacus quatuor Civitatum Sylveſtrium*, but reſumes its Courſe and Name at *Lucern*, and at laſt falls into the *Aar* below *Windiſh*, *Vindoniſſa*.

The *Aar*, *Arola*, *Arula*, ariſes upon the high Mountain *Grimſula*, in the upper *Valleſia*. About three Hours below that, it falls into the Lake of *Brientz*, and out of that, not far from the Monaſtery *Interlachen*, into the Lake of *Thun*, which it leaves near the Town of *Thun*, and thence running by *Bern*, *Solothurn*, and ſo down, falls at laſt, after many Windings and Turnings into the Rhine near *Coblentz*, *Confluentia*, probably ſo called from the uniting of theſe two conſiderable Rivers. But to proceed.

Gemmius Mons, the *Gemmi*, is a very high and ſteep Mountain in *Valleſia*, over which there is a Paſſage, but only in Summer-time, from the *Fruttinger Valley*, in the Canton of *Bern*, to the Mineral Waters at *Leük* in *Valleſia*. The Deſcent, on the South-ſide of this Mountain, is ſteep and frightful, even to the Aſpect, beyond what can be imagined, being a narrow Path, cut on the Side of almoſt perpendicular Precipices, ſometimes with trembling wooden Bridges, or Planks over the Clefts in the Mountain, and here and there ſupported with low Walls. Having been geometrically meaſured, it was found of 10110 Feet in Length, or rather Height, its many Windings and Turnings included. At a ſmall Cottage, called *Zur Dauben*, a poor reſting Place for weary Travellers, being the higheſt Part of the Mountain which is paſſable, the Mercury ſubſided *July* 1, 1709, to 21″ 3‴ which gives the Height

of

of that Place, according to

Mariotte - - -	974°	5′	or 5849′
And Dr. *Scheuchzer* -	1002	0	or 6012

Not far from this Cottage, is a ſmall mountainous Lake, called the *Dauben Sea*, or the *Pidgeons Lake*, encompaſſed on all Sides with high Mountains, the Tops whereof, for their Steepneſs, it would be impoſſible to reach. At *Kandelſtag*, the firſt Village in the *Frutinger Valley*, in the Territory of *Bern*, going up to the *Gemmi*, the Mercury roſe on the ſame Day to 24″ 2‴, which give according to

Mariotte - -	520°	1′	or 3121′
Dr. *Scheuchzer* - -	534	1	or 3205

And at *Müllenen*, at the Foot of the *Gemmi*, it ſtood at 25″ 7‴, which anſwer according to

Mariotte to - -	318°	5′	or 1913′
Dr. *Sheuchzer* - -	327	0	or 1962

On the other Side of the *Gemmi*, at *Leück*, a celebrated Place for its Mineral Waters, the Mercury was obſerved *July* 2, and *July* 5, 1709, at 23″ 9‴, which anſwers according to *Mariotte*, to 581° 4′, or 3490′, and according to Dr. *Sheuchzer*, to 597° 3′, or 3585′. So that the Cottage *Zur Dauben*, riſes above *Leück*, according to

Mariotte, - - - -	2359′
Dr. *Scheuchzer* - - -	2427

Above *Müllenen*, in the *Frutinger Valley*, according to

Mariotte - - - -	3936′
Dr. *Scheuchzer* - - -	4050

And

And the perpendicular Height of the *Gemmi*, above the Level of the Sea, conſiderably exceeds 6000 *Paris* Feet.

But high above all the Mountains of *Swiſſerland* riſes the *Stella*, *Piz Stail*, a ſteep Mountain in the *Schamſer* Valley, in *Rhætia*, or the *Griſoons*, the Height whereof was by my Uncle Dr. *John Scheuchzer*, by ſome Obſervations made in the Year 1709, determined to 9585 *Paris* Foot, above the Level of the Sea, according to his own Calculation, or 9441 according to *Mariotte*, and 12196 according to *Caſſini*: A Height, which the *Rupicapræ*, or *Shamoys* themſelves ſcarce venture to aſcend. And 'tis to theſe only, and the like Heights the following Verſes of *Silius Italicus* ought to be applied.

Cuncta gelu, canâque æternùm grandine tecta,
Atque ævi glaciem cohibent: riget ardua montis
Ætherei facies, ſurgentique obvia Phœbo
Duratas neſcit flammis mollire Pruinas.
Nullum ver uſquam, nullique æſtatis honores,
Sola jugis habitat diris, ſedeſque tuetur
Perpetuas deformis hyems ——

Having thus determined the Heights of the Mountains of *Swiſſerland* from Barometrical Obſervations, I will now cloſe theſe Remarks, which are inſenſibly grown to a much greater Length, than I at firſt intended they ſhould, with a few general Obſervations on the natural Hiſtory of that Country, ariſing from the Height of its Mountains.

The firſt Obſervation ſhall regard the Lakes, of which there are ſeveral, and very remarkable ones, in

and

and upon the Borders of *Swiſſerland*, which will afford me a ſingular Inſtance of Divine Providence. The Aſcent of the Mountains of *Swiſſerland* being ſo very ſudden and quick, that as I have above ſhewn, the Elevation of the Mountains in the Canton of *Glarus* above the Horizon of *Zuric*, though not quite three Days diſtant, is more than three Times as great, as the Elevation of *Zuric* itſelf above the Level of the Ocean, of which it is upwards of 375 *Engliſh* Miles diſtant in a ſtreight Line; and ſo in Proportion of others; and the Rivers, which ariſe in theſe Mountains, ruſhing down, in Conſequence of ſo quick a Deſcent, with great Force and Impetuoſity, it was to be feared, they would often overflow their Banks, and cauſe frequent Inundations in the flat Countries, (of which there are too many Inſtances in our own Vallies and Plains,) if this Force and Impetuoſity was not in great Meaſure broke, and their Waters diſpoſed to a more gentle Deſcent. And this is effectually done by thoſe great Receptacles of Water, the Lakes, which are beſides of infinite Uſe to the Inhabitants around them, ſupplying them with Plenty of Fiſh for their Suſtenance, and enriching them by the Facility with which Commerce may be carried on over them. Thus the *Rhine* falls into the *Lacus Bodamicus*, *Boden-Sea*, the *Rhoſne* into the *Lacus Lemannus*, or *Lake of Geneva*, the *Mueſa* and *Theſin* into the *Lake of Locarno*, the *Reüs* into the *Lake of Lucern*, the *Adda* and *Maira* into the *Lake* of *Como*, the *Lint*, or *Limat*, into the *Lake* of *Zuric*, the *Aar*, into the Lakes of *Brientz* and *Thun*. And it ſeems, that the more conſiderable the Rivers are, and the more impetuous their Courſe, ſo much the greater muſt the Receptacles be, wherein they are to looſe their

Force

Force and Rapidity. The *Lake of Geneva*, and the *Boden-Sea*, the two largeſt in *Swiſſerland*, evidently evince what I here aſſert, and the others above-named gradually decreaſe in Largeneſs, in proportion as the Rivers, which fall into them, are leſs and leſs rapid.

The extream Smallneſs of the Alpine Plants is another Obſervation, I intend to make. They become leſs and leſs, in proportion as the Mountains, upon which they grow, riſe higher. Whether this be owing to the Sharpneſs and Purity of the Alpine Air, or the decreaſing Preſſure of the Atmoſphere, which is far leſs upon Mountains than in Valleys and lower Countries, or to a Want of a ſufficient Quantity of ſubterraneous Heat, to puſh the Nouriſhment into the Roots and Veſſels of the Plants, or rather to a joint Concurrence of theſe and other Cauſes, would require a more leiſurely Conſideration. The Thing itſelf is an indiſputable Matter of Fact, and it extends alſo to Trees and Shrubs, which become ſmaller, as they grow higher. Nay, what is ſtill more remarkable, no Trees will grow beyond a certain Height, which is the Reaſon why the Tops of Mountains appear ſo bare and naked, if viewed at a Diſtance, though a curious Traveller ſhall not fail meeting upon their rich Paſtures with an agreeable Variety of beautiful Plants. The Height, where the Trees ceaſe to grow, hath been found, by Barometrical Obſervations, nearly to be the ſame in divers Parts of *Swiſſerland*. Otherwiſe, the Smallneſs of the Alpine Plants is abundantly compenſated by the Richneſs of their Virtues, which are, as it were, purpoſely centred there into ſo narrow a Compaſs.

But to another Obſervation. The Mountains are much more abrupt, and ſteep, and the Precipices greater

to the South, than to the North, and Weſtwards than Eaſtwards. Many Inſtances of this might be given in particular Mountains in *Swiſſerland*, as the *Gemmi*, the *Mons fractus* and ſo forth; but it is alſo evidently true with Regard to the whole. Thoſe are the higheſt Mountains, which ſeparate *Valleſia*, the Canton of *Uri*, and the ſeveral Leagues of the *Griſons*, from *Savoy*, *Piemont*, and the *Tirol*, which lie to the South, or South-Eaſt. Thoſe very Countries are, as it were, one continued Set of high Mountains, quite to the Mediterranean Sea, and the like Structure ſeems to be continued farther on into that Sea itſelf. The *Pyrenean* Mountains alſo are but a Continuation of that vaſt Chain, which begins in the *Lepontiæ Alpes*, or the Mountains in the upper *Valleſia*, the Canton of *Ury* and *Rhætia*, and from thence ſpreads itſelf chiefly Weſt and South. On the contrary to the Eaſt and North they break off by Degrees into gentle Plains, which appears evidently by the vaſt Tracts of Ground, which the *Rhine* for Inſtance, and the *Danube* compaſs, before they loſe themſelves, the one into the *German* Ocean, the other into the black Sea, whereas the *Rhoſne*, on the other Side, quickly and with a proportionable Velocity reaches the Mediterranean. The ſame Obſervation, with Regard to the abrupt Steepneſs of Mountains to the South and Weſt, holds true in other Parts of *Europe*, remarkably in *England* and *Norway*, more or leſs in other Countries. And ſo far as our Maps, and the Accounts of Travellers go, the ſame Thing is obſervable in other Parts of the World, but moſt evidently in the high Mountains of *Peru* and *Chili* in *South America*, which terminate very abruptly Weſtwards into the *Pacifick* Sea, but gradually decline

to

to the Eaſt into immenſe Plains, watered by ſome of the moſt conſiderable Rivers in the known World, particularly the *River of Amazons* and the *Rio della Plata*, which ariſe in the ſaid Mountains.

To conclude, from what hath been hitherto ſaid, it appears evidently, that the Mountains of *Swiſſerland* are the higheſt of *Europe*, and the great Storehouſe, whence all the Countries around them are ſupplied with Water; conform to what the learned *Loritus Glareanus* hath long ſince elegantly expreſſed in the following Verſes.

Præterea caput Europes hanc eſſe probabunt:
Æternis Alpes nivibus, juga Olympica, quorum
Porgitur in cælum caput, & ſub Tartara venter:
Et quod ad Auroram, Boream, Solemque cadentem
Flumina perpetuo non deficientia curſu
Parturit, illa volant & in omnia membra redun-
(dant,
Ad Zephyrum & Libyen Rhodanus, Rhenana fu-
(rentem
Vnda citat Boream, gelidus rotat Iſter ad Eurum
Dirus aquas, Getico novus Hoſpes & advena
(Ponto.
Aſtalios ſileo, ſileo quos Italia accipit amneis
Alpibus à noſtris, quæque alto à vertice montes
Agmina diſparibus fundunt latiſſima Sulcis.

Whether the Depths of the Sea correſpond with the Heights of Mountains, muſt be left to future Obſervations.

II. *Optical Experiments made in the Beginning of* Auguſt 1728, *before the Preſident and ſeveral Members of the Royal Society, and other Gentlemen of ſeveral Nations, upon Occaſion of Signior* Rizzetti's *Opticks, with an Account of the ſaid Book, By* J. T. Deſaguliers, *L. L. D. and F. R. S.*

SOme Time in the Year 1722, Signior *Gizlanzoni*, an *Italian* Gentleman, ſhew'd me a Paper of Signior *John Rizzetti*, wherein he denied the different Refrangibility of the Rays of Light, becauſe an Experiment mentioned in Sir *Iſaac Newton*'s Opticks (B. 1. *Prop.* 1. *Exp.* 2.) concerning an oblong Paper painted half blue and half red (whoſe Image projected by a Lens upon a white Paper at a conſiderable Diſtance became diſtinct in its blue half, nearer the Lens than in its red half) had not ſucceeded with him, tho' after many Trials. Sir *Iſaac Newton* being acquainted with this, deſired me to repeat his Experiment abovementioned, which I did at my own Houſe, before him and Signior *Gizlanzoni* and ſome other Perſons, who were ſatisfied with the Succeſs of it, according to Sir *Iſaac Newton*'s Aſſertion. Afterwards on the 13th of *December* 1722, I again repeated the Experiment before the *Royal Society*, with the ſame Succeſs, the full Account of which is printed in the *Philoſophical Tranſactions*, Number 374. After this, Signior *Gizlanzoni* read me a Letter from Signior *Rizzetti*, wherein he ſaid; " He wanted to know whether the

" Expe-

Fig. 1.

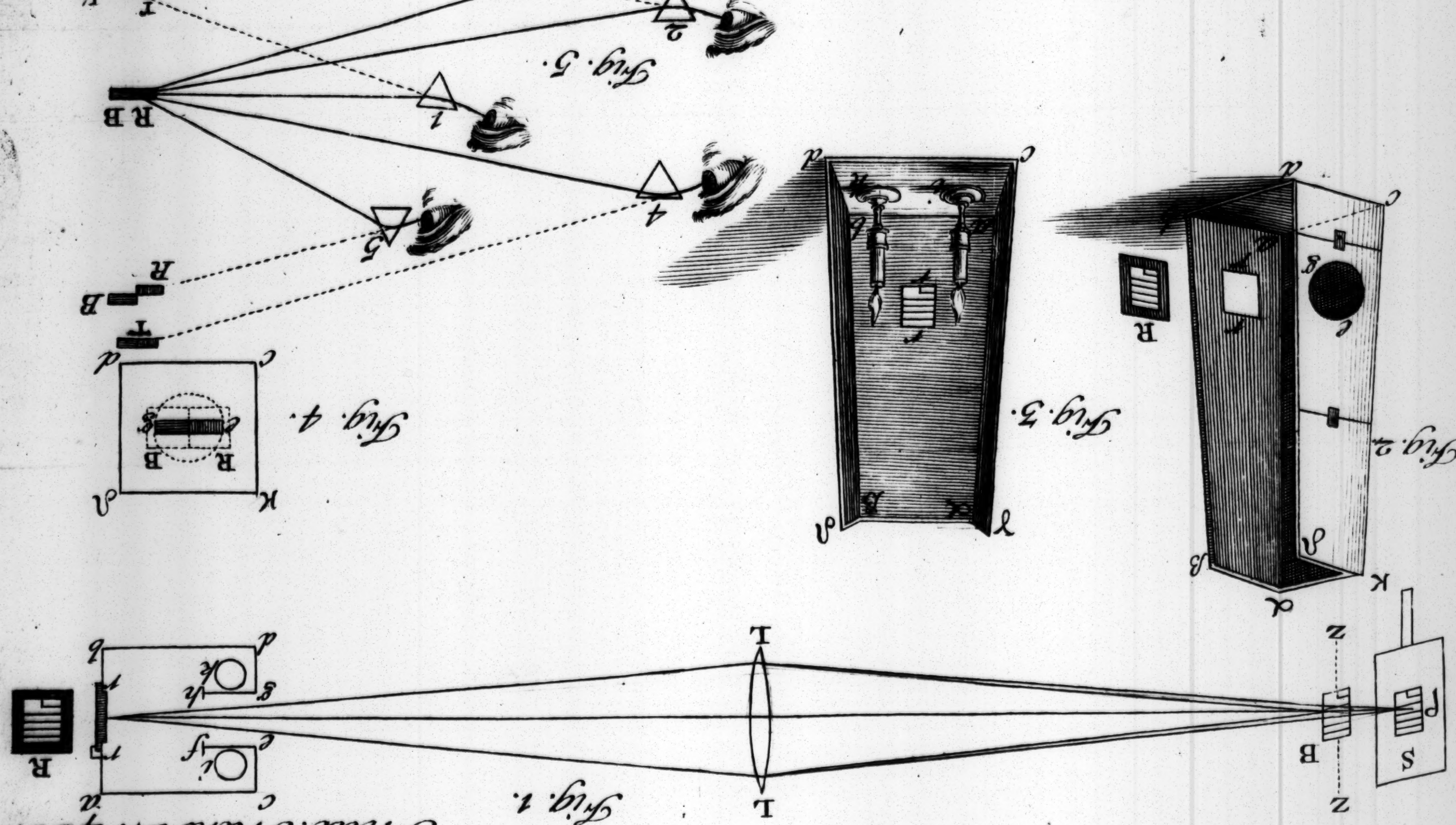
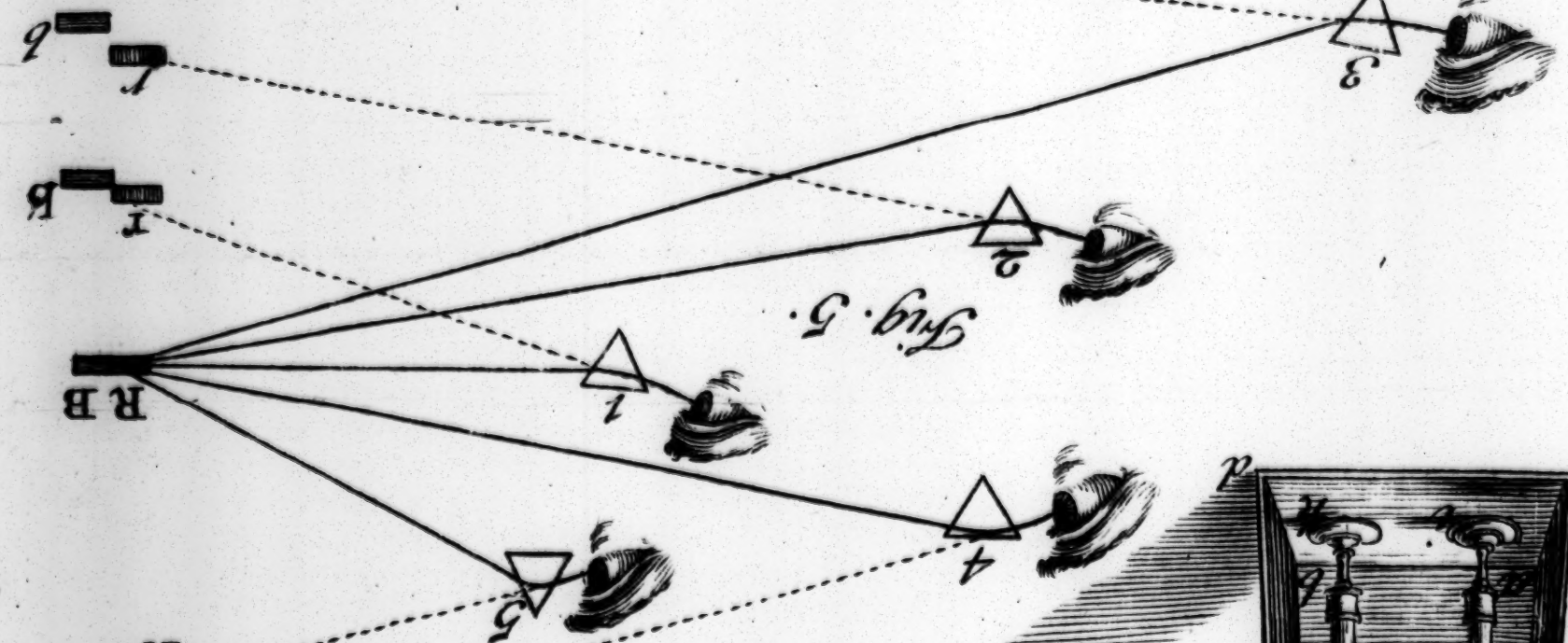


Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.



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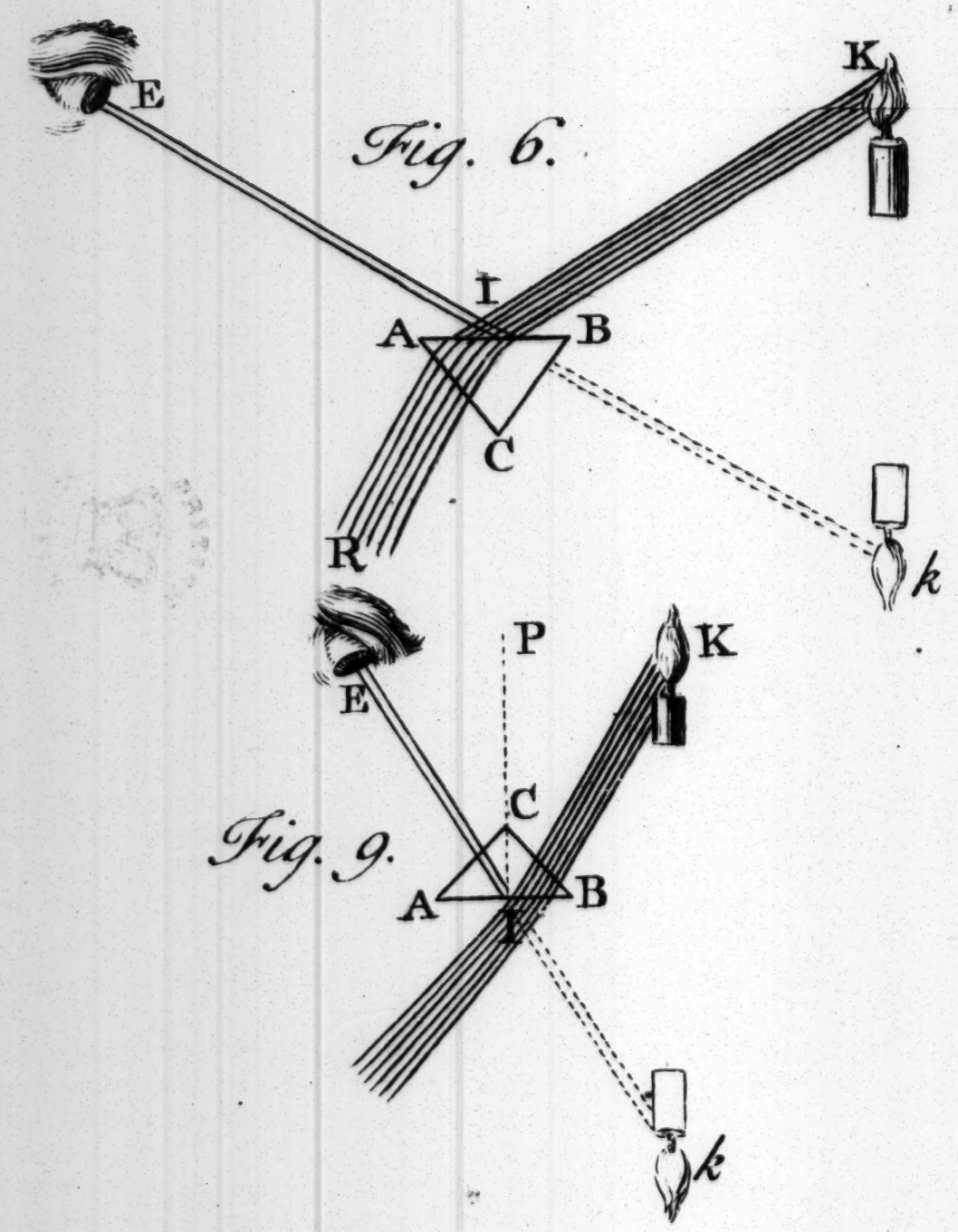


Fig. 6.

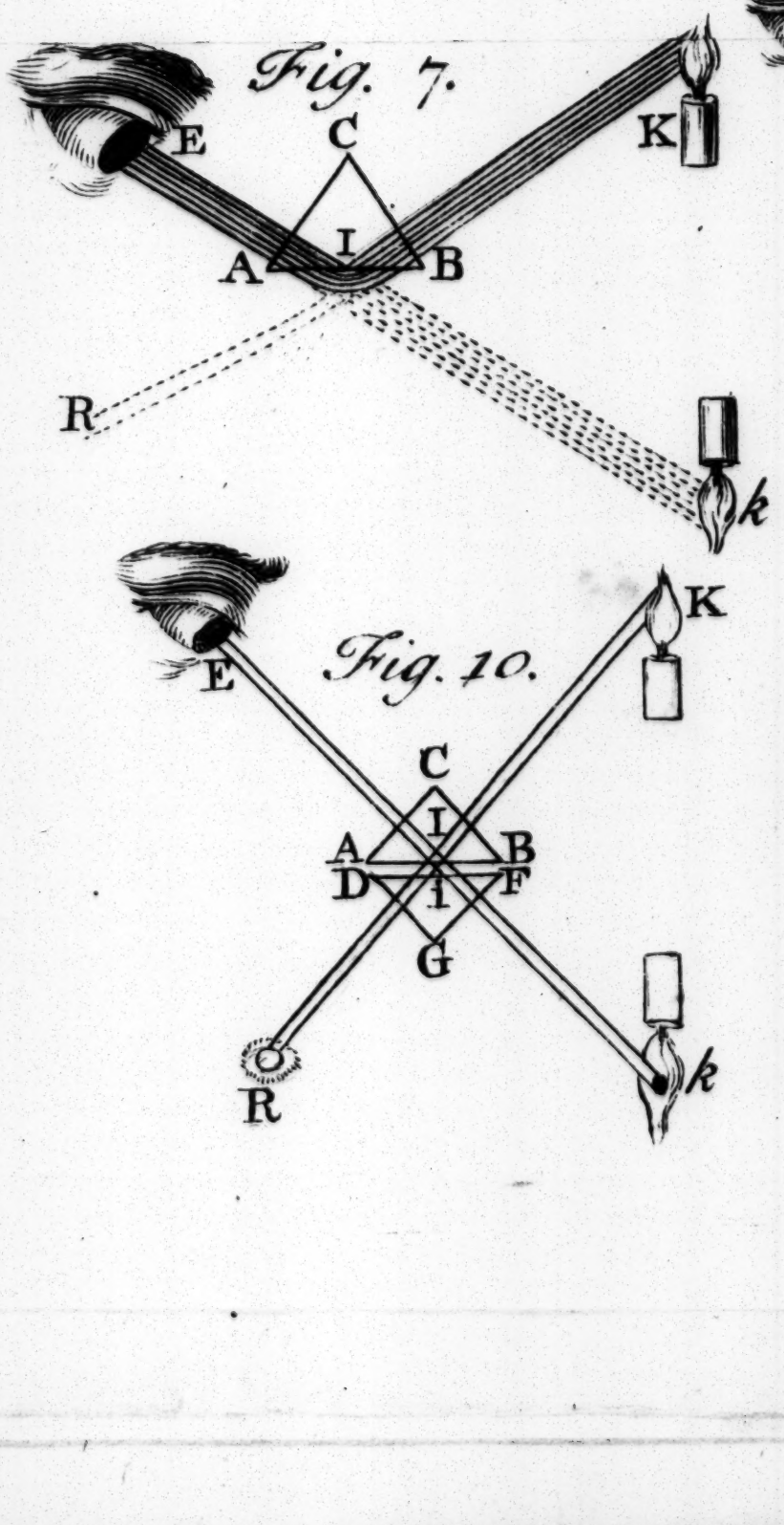


Fig. 7.

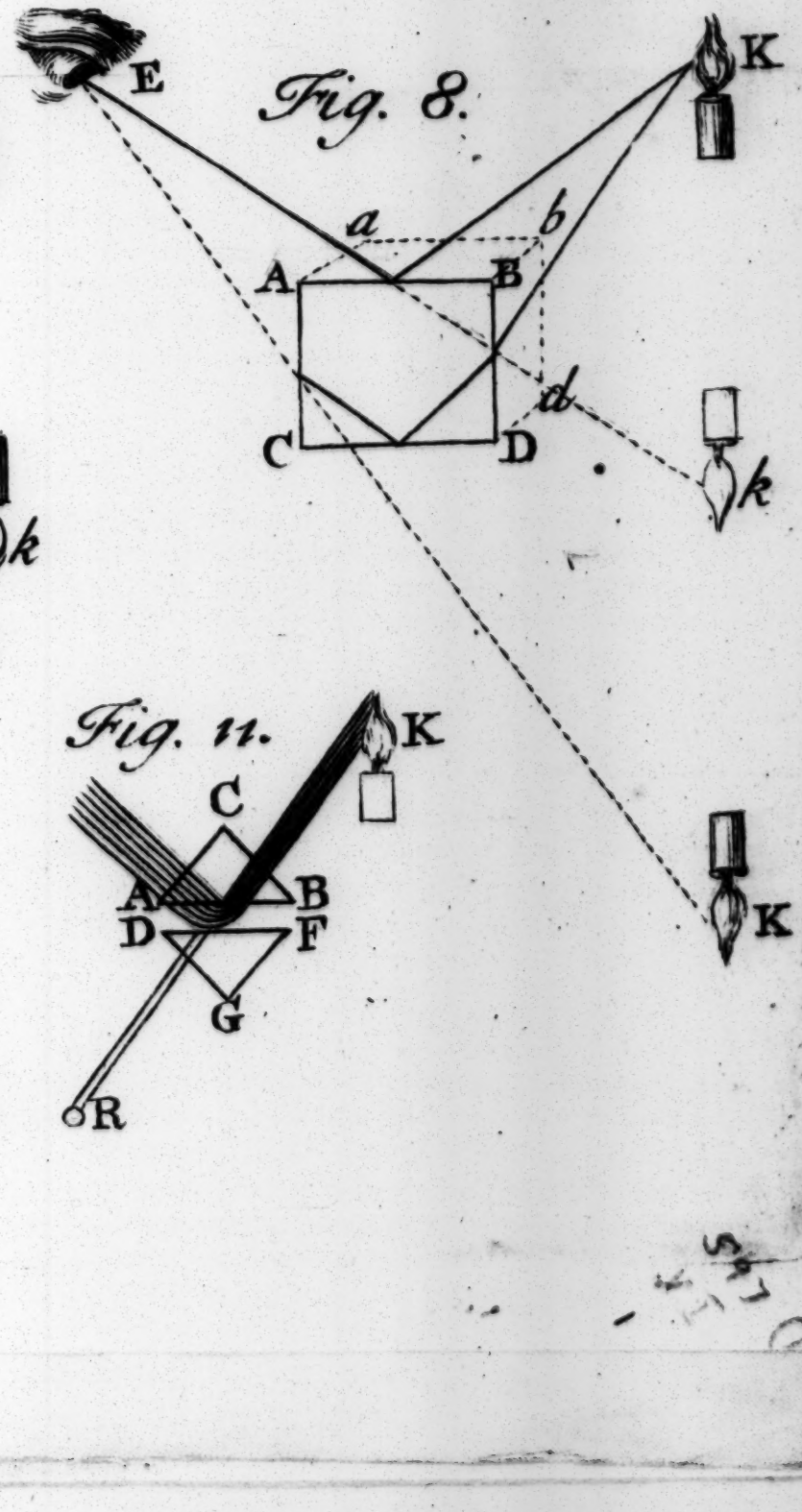


Fig. 8.

Fig. 9.

Fig. 10.

Fig. 11.

Fig. 12.

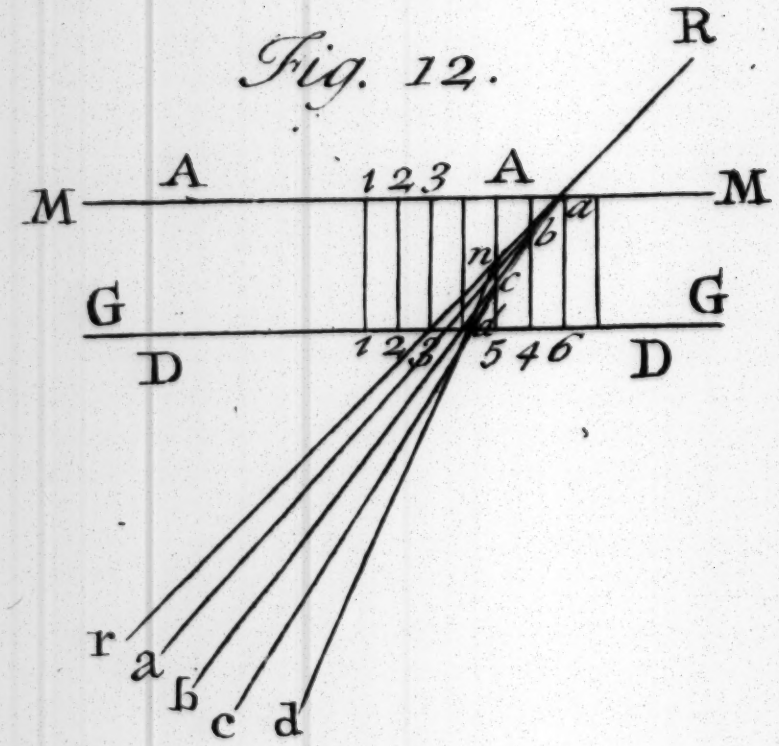


Fig. 13.

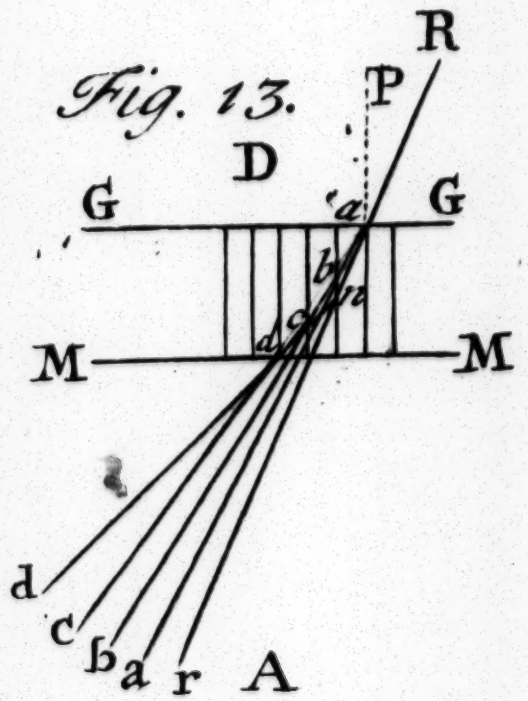
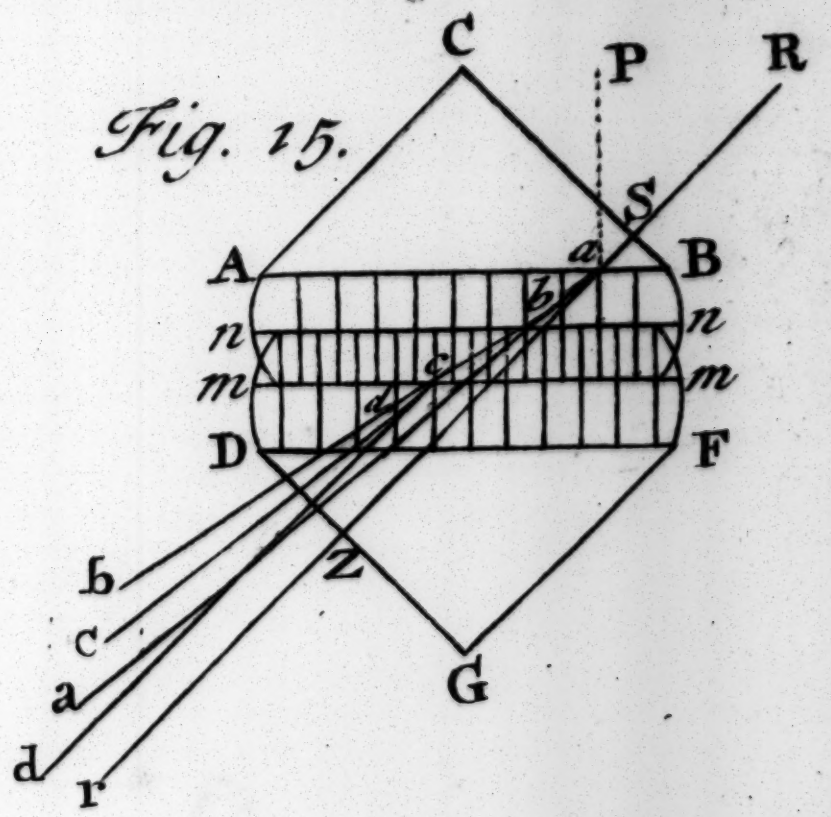


Fig. 15.



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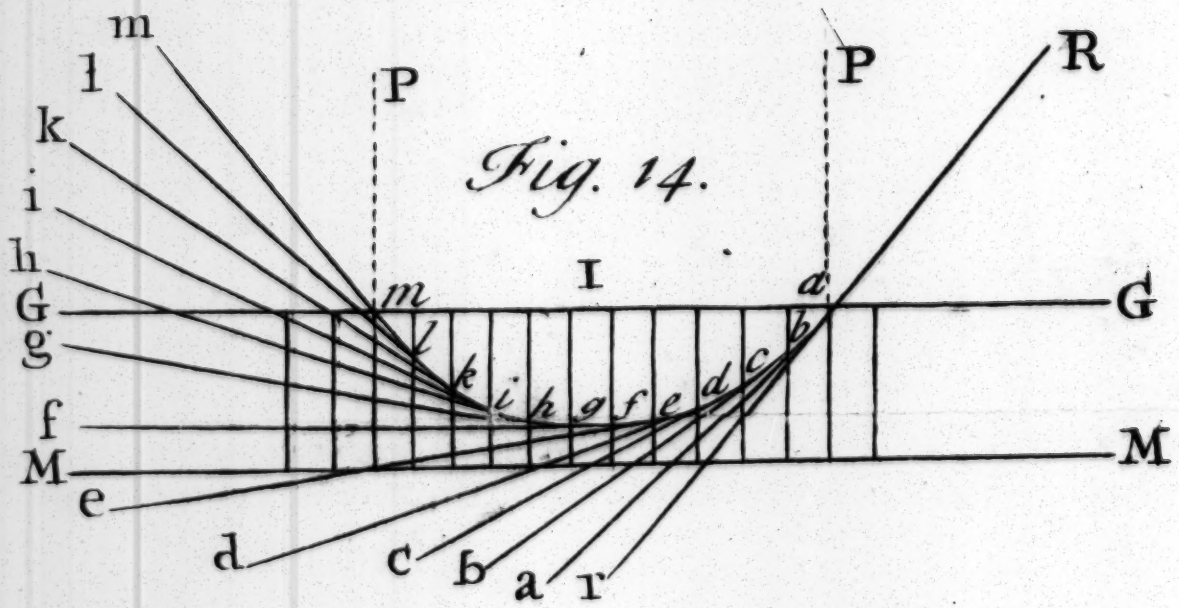


Fig. 16.

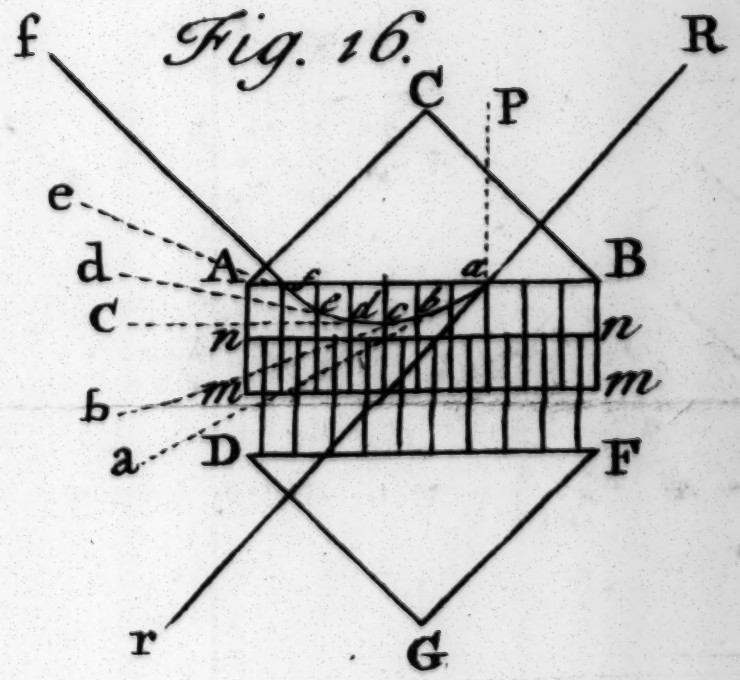
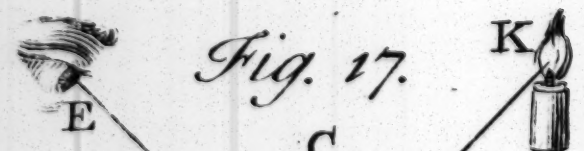


Fig. 17.



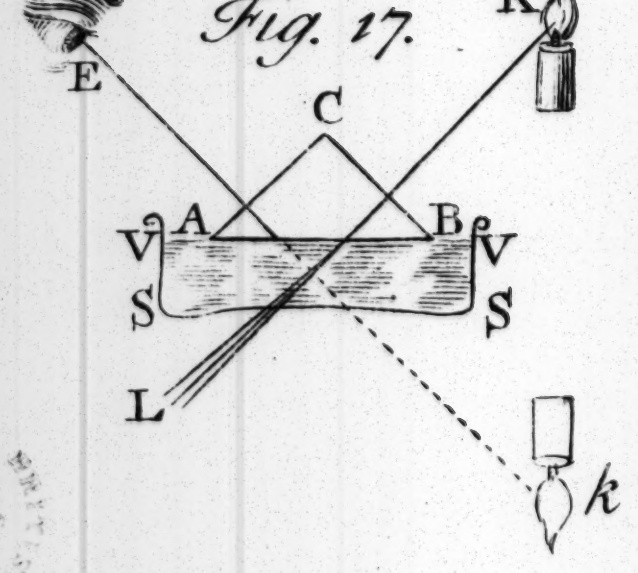


Fig. 17.

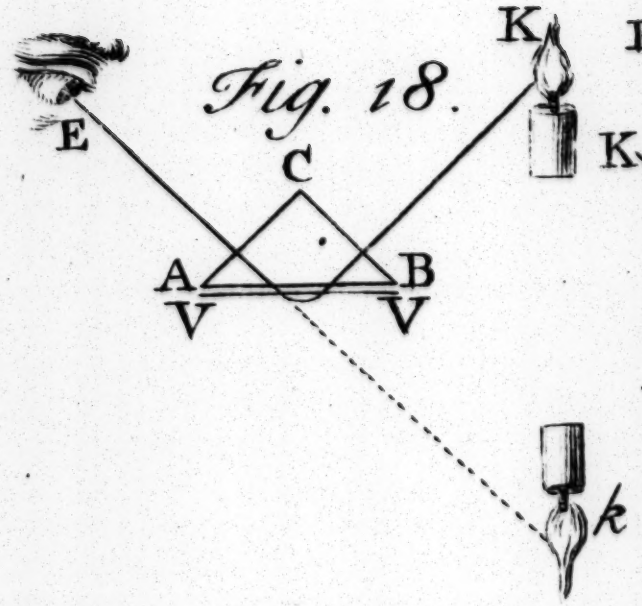


Fig. 18.

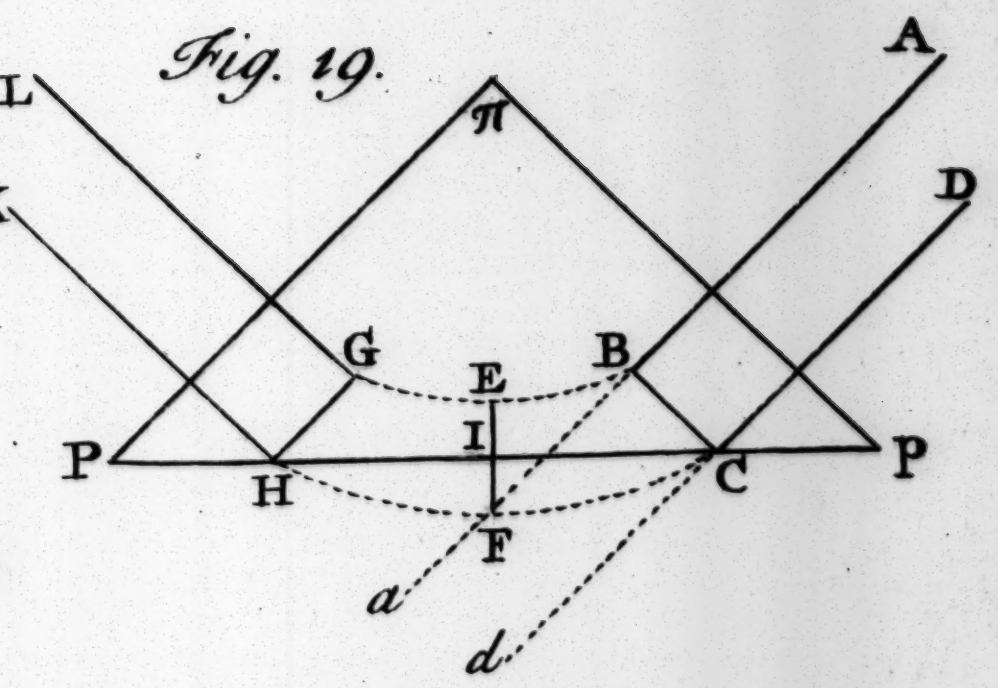


Fig. 19.

Fig. 20.

Fig. 21.

Fig. 22.

" Experiment would ſucceed, if the Paper was turned " ſo as to bring the red Half in the Place of the blue " one; and that if it ſucceeded then, yet he would " not acquieſce, but he ſhould have ſomething ſtill to " object againſt it. And further, that he wanted to " know what could be ſaid to ſeveral other Objections " (I think there were twelve in the Paper ſhewed me) " againſt many more of Sir *Iſaac Newton*'s Optical " Experiments, the greateſt Part of which he ſaid, " he had found to ſucceed differently from what " Sir *Iſaac* had related; and would not allow the " Conſequences to be juſt, which were drawn from " the other Experiments which he had found to agree " with his Trials". Upon this I acquainted Signior " *Gizlanzoni* in a Letter, which I deſired him to com- " municate to Signior *Rizzetti*; " that as Signior " *Rizzetti* had put the Iſſue of the Diſpute upon the " Succeſs of an Experiment, which after repeated Trials " had ſucceeded contrary to his Opinion, he ought to " acknowledge his Miſtake; and then I ſhould willingly " repeat all the other Experiments which he had called " in Queſtion, and endeavour to remove his other Diffi- " culties. That if it was Truth and not Victory which " he contended for, I did not doubt but he would com- " ply with me, in what I inſiſted upon; and then I ſhould " be ready to make any Experiment, or clear up any Dif- " ficulty relating to the Doctrine of Colours, in the beſt " manner I could". But I never heard any more from Signior *Rizzetti*; but was told by others that he was very angry at Signior *Gizlanzoni*, and ſaid he was got into Sir *Iſaac Newton*'s Party.

Now at laſt Signior *Rizzetti* has publiſhed a Book, entituled *De Luminis Affectionibus Specimen Phyſico-mathematicum*,

mathematicum, dedicated to Cardinal *Polignac*, and printed at *Treviſe* and *Venice*, 1727, which being preſented to the *Royal Society*, and by the Society recommended to me to give an Account of it, I hope no Body will blame me for making a faithful Report.

The Author in his Preface, and throughout the whole Book, in a moſt arrogant manner, has inſulted the greateſt Philoſopher that this or any other Age ever bred, triumphing in what he thinks the Miſtakes of Sir *Iſaac Newton* and his own Diſcoveries. Had he modeſtly related the Facts as they appeared to him, and his Reaſons for drawing Conſequences different from thoſe of Sir *Iſaac Newton*, the World might have thought him urged on by the Love of Truth in his *ten Years Labours*, * and his Errors might have been excuſed according to what he ſays in his Preface *Si fortè decipior, haud turpis eſt in re Phyſico-mathematica error, & magnis ſe quiſque tuetur exemplis.* Neither would his Fame have been the leſs (if he had been right in his Experiments and Reaſonings) for treating his Adverſaries in a civil Manner, and really doing what he ſays at the End of his Preface " *Placita* " *quidem authorum laceſſo; at ipſos tamen authores* " *obſequio & veneratione proſequor*": For ill Manners can never be excuſed by what he calls *Philoſophica Libertas.* Now nothing leſs than owning, *that a greedy Deſire of Fame, and an Obſtinacy to maintain what he once laid down as his Opinion, miſled him ſo far, can excuſe him to the learned World.* Wehear indeed in a Letter from Sir *Thomas Dereham* to Sir *Hans Sloane*, Preſident of the *Royal Society*, that *now* Signior *Rizzetti* alledges, " That he was

* See Preface, p. 38.

deceived

" deceived in his Experiments, by Reaſon of the Bad-
" neſs of his Priſms which he had from *Venice*" ; but this is but a partial Acknowledgment of his Error, and only ſatisfactory to ſuch as have not read his Book, and are likewiſe unacquainted with Sir *Iſaac Newton*'s Opticks. For Signior *Rizzetti* in the 37th and 38th Pages of his Preface has theſe Words—" *Ut ſuoſmet*
" *oculos conſulant, omnes quidem poſſunt mea Ex-*
" *perimenta iterare; at ſi primo intuitu deficit, qui*
" *expectatur, eventus; ſtatim me falſitatis non ar-*
" *guant (ut vir celebris dicitur nimia feſtinatione*
" *feciſſe); ſed prius omnes meorum experimentorum*
" *limitationes notent: Si ad has enim attenderint;*
" *non pingui Minerva, non trigonis imperfectis [ut*
" *alii dictitant] ſed accurata diligentia, & inſtru-*
" *mentis idoneis me in hujuſmodi res incubuiſſe etiam*
" *adverſarii, vel inviti fatebuntur*". As to Sir *Iſaac Newton*'s Experiments, a great many of thoſe which Signior *Rizzetti* calls in Queſtion may be performed with very ordinary Priſms: And of that Sort are thoſe which I made ſince the Publication of the Book in the Beginning of laſt *Auguſt*, at my own Houſe, before the Preſident and ſeveral Members of the *Royal Society*, and ſome foreign Gentlemen. But before I give an Account of the Experiments, I beg Leave to quote ſome of Signior *Rizzetti*'s Expreſſions againſt Sir *Iſaac Newton*, otherwiſe I may be thought guilty of the very Fault which I have laid to his Charge.

Preface, Page 13. " *Miratus ſum quod acutiſſimus*
" *Newtonus ignoraverit Lumen album aliter a*
" *Trigono, & aliter a lente diſpergi.*" Who ever read Sir *Iſaac Newton*'s Opticks, that can be of this Opinion, except our Author ? Preface, Page 81.—

" *Theoria*

" *Theoria in qua tot Hypotheſes ſunt, quot expli-*
" *canda Phænomena*". When it is well known that Sir *Iſaac Newton* did not make Hypotheſes to explain, but deduced plain Conſequences from *Phænomena*. In the Book, Page 55, he ſays of *Richterus* his Adverſary—" *Si diverſam colorum refrangibilitatem*
" *garriendo tueri deliberat* ",—and of Sir *Iſaac*....
" *Si Newtonus hallucinatus eſt, quia in uno experi-*
" *mentorum genere fallaciis unici ac difficillimi in-*
" *dulſit ; nos antequam conſequentias eliciamus,*
" *diſcimus omnia ejuſdem generis experimenta*
" *peragrare, a facilioribus incipiendo.*" Page 76. ſpeaking of Sir *Iſaac*—" *Nunc igitur confidentius*
" *affirmo, quod equidem plures, quam debuerat con-*
" *ſequentias ex Phænomenis auctor collegit.*" Page 90. " *Quis auſiſſet canonem tanti viri impugnare,*
" *cum ceſſerunt omnes, qui hactenus eum veluti*
" *trutina expenderunt*, &c." Page 91. " This Ex-
" preſſion is particularly remarkable—" *Newtonus*
" *hoc argumentandi modo videtur feciſſe ut Lesbii,*
" *qui, cum ædes ad Regulam minime accomoda-*
" *rentur, Regulam ad ædes accomodarunt*".

I cannot paſs by what he ſays of *Richterus* in p. 100,
" becauſe it is ſo applicable to himſelf, *Richterum*
" *admonui, ut marte ſuo, quod omiſerunt alii per-*
" *ficeret ; is autem, ſpreta admonitione, videre*
" *quod a natura agatur, ſed eam potius agere vult,*
" *quod ſibi videtur agendum.*" Page 127. " *Cum*
" *hæc auctor in medium ferat, parum in iis exper-*
" *tus eſſe videtur, quæ in poliendis vitris occurrunt.*"

Not to be tedious in ſuch unpleaſant Quotations, I proceed to give a ſhort Account of the Book it ſelf, to ſhew how much Pains ſome People can take to be in the Wrong,

Wrong, there being no Experiment of Sir *Iſaac*'s called in Queſtion but what is true, and no Conſequence different from Sir *Iſaac*'s in thoſe Experiments which he found to ſucceed, but what is falſe; as may be evident to any one who reads Sir *Iſaac Newton*'s Opticks with Attention enough to underſtand them; and has proper Inſtruments and ſufficient Dexterity and Accuracy to perform the Experiments.

Our Author (tho' he profeſſes himſelf an Enemy to Hypotheſes) begins his very firſt Propoſition with a Demonſtration drawn from a falſe Hypotheſis; for he ſuppoſes every Beam of Light to be as a Parallelogram of ſome Breadth like a Ribbon, as if the Rays cohered together like the longitudinal Threads of the Ribbon; then conſidering a narrow Side of the Parallelogram (which he calls the Section of the Beam) as an inflexible Line, he takes a great deal of Pains to draw a Conſequence from it, which Experiments ſhew to be falſe; namely, that Light paſſes with more Difficulty through a denſe, than through a rare Medium. He affirms, "That white Light never affords Colours by Reflexion."

That the Union of all Kind of Rays do not make "*white*."

"That Light reflected from a *white* Object, "and ſeen through a dark Medium, becomes *yel-* "*low* or *red*, as the Medium is ſtronger or weaker; "that *black* ſeen through a lucid Medium appears *blue* "or *violet*; and *green* he ſays is made from a

black } Object ſeen { lucid } then thro' a { dark } Medium.
white { firſt thro' a { dark, } { lucid }

"That ſome Light paſſing through a refracting "Medium is diſperſed, which he calls *more than re* "*fracted*, and ſo produces Colours."

In order to ſupport his Hypotheſes of *Mediums like Veils to alter the Colours of Objects looked at*, " he " conſiders (Preface, Page 31.) the Images in the Eye " as an Object looked at, which would be ſuppoſing " other Organs of Viſion to look into the Eyes"—— whereas the Pictures of external Objects ſhewn upon the back Part of an Eye placed in an Hole in the Window of a dark Room, are only ſuch to thoſe who ſee the Experiment; but in the Animal who ſees, thoſe Pictures are a great Number of ſmall Blows or Impreſſions made upon the Fibres of the *Retina* by the Impulſe of many Rays collected in the Vertices of the Cones of Light within the Eye, correſponding with ſo many other Cones which proceed from the viſible Points of external Objects, and make, what Opticians call Pencils of Rays.

" That the Reſiſtance of Water from its Tenacity is " greater than from its Denſity.

" That ſince a ſmall Thread, half *blue* and half *red*, " is ſeen diſtinctly by the naked Eye, that Phænome- " non overthrows the Doctrine of *different Refran-* " *gibility*." But here the Author does not conſider, that the Focus of the Eye is ſo ſhort that the Diſtance of the diſtinct Baſe of the *blue* and the *red* Image of ſuch Threads is not equal to the Thickneſs of the *Retina*.

" That the Experiment of the two-coloured Paper " projecting its Image through a Lens (which I repeat- " ed in the Manner above-mentioned, in the Year " 1722) did ſometimes ſucceed with him, and ſome- " times not; and therefore that it did not prove the dif- " ferent Refrangibility of Rays: But the different " Place of the diſtinct Baſe of the *blue* and the *red* " Image, was to be aſcribed to the different Inclina-

" tion

" tion of the Parts of the painted Paper to the Sur-
" face of the Lens." But in my Account of the Experiment in the *Philoſophical Tranſactions*, I mentioned particularly that the Axis of the Lens was perpendicular to the Image of the Card, and therefore there could be no different Obliquity, as is objected.
" That though he found the *Spectrum* of Colours
" produced by the Priſm in a dark Room to ſhorten by
" Degrees, and at laſt become round and colourleſs
" (that is *white*) when viewed by another Priſm, in
" the ſame manner that Sir *Iſaac Newton* had made
" the Experiment; yet it did not convince him of the
" different Refrangibility of the Rays; becauſe when
" he had cauſed an Image to be painted upon a Paper
" like the *Spectrum* from the Priſm, and enlightned it
" by the direct Light of the Sun, it did not become
" round and white when viewed through a Priſm as
" the other *Spectrum* did." But he did not conſider the Imperfection of Painters Colours, nor remember that the Surfaces of Bodies, whether of a natural, or a painted or died Colour (ſuch as he calls *permanent Colours*) when expoſed to any coloured Light, will reflect that Colour which falls upon them, and appear to be of no other, only that they will ſeem moſt vivid in that Colour which they bear in openDay; and therefore that if the Sun's Light conſiſts of Rays differently refrangible and producing different Colours (according to Sir *Iſaac Newton*) the Priſm muſt ſeparate the Light reflected from every one of the painted Colours, and could not bring them together, becauſe they were by no means ſimple Colours. If therefore he had reaſoned right, the firſt Experiment would have proved Sir *Iſaac*'s Doctrine, and the laſt would have

confirm'd it. And if in his own Experiment he had looked at the painted *Spectrum* ſucceſſively holding the refracting Angle of the Priſm upwards, and then downwards with the ſame Inclination (or, what is eaſier, turned the *Spectrum* upſide down, the Priſm remaining fix'd) he would have ſeen his *Spectrum* ſhorter in one Caſe than in the other.

" That Sir *Iſaac Newton*'s 8th Experiment of P. I. " B. II. (in which the priſmatick *red* and *blue* falling " ſucceſſively on the ſame Place of a Book, have " a different Focus in projecting their Image through a " Lens) is inconcluſive; and rejects *Richterus*'s " Anſwer, *viz. That the Colours reflected from the " Book, as it has a rough Surface, fall always with " the ſame Inclination upon the Lens, in what- " ever Direction they came from the Priſm to the " Book* —— adding, That he had acquainted " *Richterus*, that *permanent* Colours enlighten'd by " direct Light in any different Inclination, always fell " upon the Lens with the ſame Inclination; but ap- " parent Colours, which were produced by Refraction " with a Priſm, differed from permanent Colours in " their Incidence: But that *Richterus* had purpoſely " concealed it."

N.B. It is not likely he would have concealed it, if it had been true.

" That Monſieur *Marriotte*'s Experiments diſprove " the different Refrangibility of Colours"; though, if he had read with Attention and Impartiality the Account of the Experiments which I made before the *Royal Society* by their Order on that Occaſion (*Phil. Tranſ.* Numb. 348.) he might have been convinced as well as ſeveral Gentlemen of the *French Royal*

Academy, who had ſeen Monſieur *Marriotte* make his Experiments, and acknowledged themſelves ſatisfied when they ſaw me repeat thoſe of Sir *Iſaac* in the Year 1715.

" That in the firſt Experiment of Sir *Iſaac* (Book I. " Part II.) He never could deſtroy any one Colour, the " reſt remaining; and that with a larger Obſtacle " he could deſtroy the *yellow* and *blue*, but not the " *yellow* and *green* — could bring the *green*, not the " *yellow*, to be next to the Shadow—and could leave " the *green* only remaining, but not the *yellow*." This is an eaſie Experiment; but Inaccuracy and a very bad Priſm, and Prejudice towards an Hypotheſis, or againſt an Adverſary, may miſlead a Man ſtrangely.

" That when the Colours produced by the Priſm, " and afterwards united by the Lens, produce *white* " upon a Paper in the Focus, no Inclination of the " Paper will tinge the white Spot with Colours".

" That a *yellow* Paper in the *blue* Light appears " *green*, as does alſo a *blue* Paper in the *yellow* " Light."—But not when the Room is well darken'd, and the Light homogeneal.

" That Sir *Iſaac Newton* aſſerts falſly, that Light " immerging into a Parallelipiped, and then emerging " out of it, produces no Colours."

" That the 6th Experiment of Part I. Book I. of " Sir *Iſaac* is true; but no different Refrangibility of " Rays is prov'd by it, though the Colours coming " ſucceſſively from the firſt Priſm to the ſecond with " the ſame Incidence, are carried to different Heights " by the laſt Priſm." Now the Conſequence is ſo plain here, that this is, after my Lord *Bacon's* Manner, called an *Experimentum Crucis*.

I ſhould

I ſhould go on with my Account of this Book, if it could be of any Uſe either to vindicate Sir *Iſaac Newton*, or to convince the Author and his Adherents, if he has any. But Sir *Iſaac*'s Opticks need neither Defence nor Explanation. And when the Author is in the Humour to be convinced, *ten Months* well employed in reading Sir *Iſaac*'s Book, will make him amends for his *ten Years* prejudiced Examination.

I beg Leave now to give the particular Deſcription of a few Experiments I made upon this Occaſion, ſome of which are exactly as Sir *Iſaac Newton* made them: Some are his, but made ſomething differently, and ſome altogether my own.

EXPERIMENT I. FIG. I.

I prepared a Box of about three Foot high, and one Foot wide within (whoſe Shape was a truncated Pyramid) in the following Manner. I painted the Inſide of it black, and in the back Part, one Foot above the Baſe, made a ſquare Hole of three Inches in Width (whoſe Section is *r r*) to receive a Piece *R* ſhutting cloſe with a Rabbet or Shoulder, whoſe Surface coming through the Hole was wholly covered with the painted Paper, on which the Experiment was to be made. Over againſt *r r*, in the fore Part of the Box, was a Door to open with a Tube in it, four Inches wide and five Inches long, whoſe Section is *e, f, g, h*, that two Candles ſet on the Places *i, k*, to enlighten the Paper at *r, r*, might throw no direct Light out of the Box, whoſe Section is repreſented at *a b c d*. Then having made the Room perfectly dark, I fixed the Box upon a Table, that it might remain in one Place; at the Diſtance of

eight

eight Foot from *r r*, I fixed the Lens L L, of four Foot Focus, in a Frame upon another Table, with its Axis going through the Middle of *r r*: At the Diſtance of about eight Foot beyond the Lens, I ſet up the Skreen or Square of white Paper S. Having put into the Hole *r r* a ſtiff Paper, painted with Vermillion, and wrapped four Times and an half with black Silk (as repreſented by R), that Paper enlighten'd by the Candles at *i*, *k*, the Image of the red Paper was projected upon the Skreen at ρ, and when the moſt diſtinct Place was found, the Skreen was fixed: Then a Paper painted with Ultramarine being fixed in the Hole *r r*, the Image of it was ſo indiſtinct at ρ, that the Images of the black Silks could not be ſeen; but holding a Piece of Paper cloſe to the Skreen, and bringing it forward, at about ¾ of an Inch from the Skreen, the Repreſentation of the Silks began to appear on the blue Image; but it was moſt diſtinct at an Inch and ¾, or at Z Z; ſo that there was 1¾ Inch between the diſtinct Baſe of the *red*, and that of the blue Paper. But what has led ſeveral People into an Error in making this nice Experiment, is the Depth of the Focus of the Rays in both Caſes; for though the red Image was moſt diſtinct at ρ, yet the Repreſentation of the black Silks might juſt be perceived by a good Eye when the Skreen was moved backwards or forwards ¾ of an Inch: The blue Image which was ſtronger had its Silks viſible an Inch on either Side of Z Z; ſo that in a Paper half red and half blue, painted with theſe Colours, one might have ſeen the Silks (though faintly) upon the two Images at once, and have been thereby deceived: But ¾ of an Inch beyond the Place common to both, the *red* alone would have appeared diſtinct; and an

Inch

Inch ſhort of the ſaid Place, the blue Image moſt diſtinct, and diſtinct alone; that is an Inch and $\frac{1}{4}$ nearer the Glaſs. Inſtead of *Vermilion* the red Paper may be painted with *Carmine* or *Lake*, but it will not do ſo well, as was then tried; nor does *Pruſſian Blue* do ſo well as *Ultramarine*. The beſt Way is to heighten the *Vermillion* with a little *Carmine*, and the *Ultramarine* (which has too much *white*) with *Indigo*; and then there will be a Space between the two diſtinct Baſes where both the Images will be indiſtinct.

N. B. *I made the Experiment with ſuch Colours, in the Year* 1722; *but now I uſed no Mixtures, that any Body elſe might repeat the Experiment.*

The ſecond Figure repreſents the Box with one Side out, whoſe Place is *δ d b B*; *e g* is the Hole for the Tube in the Door of the Foreſide, *x δ c d*; *r r* the Hole in the Back to receive the Piece R with its painted Paper.

The third Figure is the Box open before, with the Candles and Paper in it, the ſame Parts being marked with the ſame Letters as in the other Figures.

N. B. I made the Experiment in this Manner, becauſe Signior *Rizzetti* attributed the different *Foci* of the Colours to different Inclinations, which could not be alledged here; the *red* and *blue* being, as he had deſired, ſucceſſively fixed in the very ſame Place: And he ſays (Page 64.) *addidi permanentes colores a lumine directo diverſa inclinatione illuſtràtos conſtante Inclinatione in lentem incidere.* Nay, more than this was performed in the Experiment; for as the Candles were fixed, the Light fell upon the painted Paper always with the ſame Incidence.

EXPE-

EXPERIMENT II.

Inſtead of the red or blue Paper at r r (*Fig.* 1, 2, and 3,) I fixed upon the Piece R, a Paper half *red*, and half *blue*, as R B (*Fig.* 4.) then over the Hole in the fore Part of the Box repreſented by *e g* (*Fig.* 2.) I fixed a ſquare Plate *α β c d* (Fig. 4.) with an oblong Hole in it four Inches long in its Horizontal Poſition, and one Inch deep, through which one might ſee the parti-coloured Paper, as if it was only of the Bigneſs and Figure of this Aperture, and ſtrongly enlighten'd by the Candles hid in the Box; the reſt of the Room being very dark. N. B. *I made this Preparation, becauſe* Rizzetti *objects to Sir* Iſaac Newton's *firſt Experiment of the firſt Book, that the black Cloth beyond the parti-coloured Paper, was not colourleſs, and therefore the Experiment was not deciſive as particularly relating to the Paper.*

R B (*Fig.* 5.) is the Paper contracted in Length and Breadth by the Aperture of the Plate, which Paper being looked at, at the Diſtance of five Foot, by the Priſm 1, appeared as drawn at r b. The Priſm being removed to 2, at the Diſtance of ten Foot, ſhewed the Paper as at *r b*. And when it was at 3 (at the Diſtance of fifteen Foot) the Paper appeared as ρ β. In theſe three Caſes the blue b, *b*, and β appeared lower than the red r, *r*, ρ, the refracting Angle of the Priſm being downwards. When the refracting Angle was held upwards, as at 5, then the *Blue* B was raiſed higher than the *red* R; but if due Care be not taken, in turning the Priſm, a Reflection may be miſtaken for a Refraction, as at 4; and then indeed the *Red*

and *Blue* will be equally raiſed as at T. This muſt have been Signior *Rizzetti*'s Miſtake, when (in *Page* 38.) he ſays that one Colour was raiſed higher than the other by two Lines, at ten Foot Diſtance, but not at all at five Foot; for ſeveral of the Perſons preſent at my Experiments, made the ſame Miſtake at firſt before they could perform the Experiment in manner above-mentioned; which they at laſt did, and found the Colours ſeparated moſt at the greateſt, and leaſt at the leaſt Diſtance. This miſtaking a Reflection for a Refraction, has been the Occaſion of ſeveral more Errors, and Difficulties to be met with in Signior *Rizzetti*'s Book.

EXPERIMENT III. FIG. VI.

A Candle K, reflected from the Surface A B of the Priſm A B C, appeared very faintly to the Eye at E, as a weak Image at k; becauſe the Rays incident at I, paſs moſt of them through the Priſm, and go on to R, ſeparating from one another according to their different Degrees of Refrangibility; whilſt a few of them are reflected to the Eye in the Direction I E.

But if the Priſm be in the Poſition A C B (*Fig.* 7.) moſt of the Rays of the Candle K, incident at I, on the Plane A B (after having paſſed perpendicularly through the Plane B C) are reflected, and paſſing perpendicularly through A C, go into the Eye at E, which ſees a very ſtrong Image of the Candle at k, whilſt very few Rays go down to R to produce Colours.

This ſhews that the Rays of Light paſs with more Facility through Glaſs (a denſe) than through the Air (a rare) Medium; contrary to *Rizzetti*'s Aſſertion.

EXPERIMENT IV. FIG. VIII.

To make this more evident, and compare together the Facilities with which Light paſſes through the two Mediums, I took a Cube of Glaſs of three Inches the Side, A *a b* B *d* D C, whoſe Section is A B C D, and looking upon it from E to ſee by Reflection the Candle K, I ſaw two Images of it; one at *k* very faint, and reflected from the upper Surface A B, and the other at *x* very ſtrong, and reflected from the lower Surface C D. Now it is evident, that the Vividneſs (or Brightneſs) of the Image *x*, is to the Vividneſs of the Image *k*; as the Facility with which the Rays, in theſe Circumſtances paſs through the Glaſs, or through the Air: and thoſe are eaſily compared, becauſe both the Images are ſeen at once.

EXPERIMENT V. FIG. IX.

The Line P I being perpendicular to the reflecting Plane A B of the Triangle A C B, I brought the Candle K by Degrees ſo near to P, as to diminiſh very much the Angle of Incidence K I P, which made the Image or Appearance of the Candle at *k*, become fainter by Degrees, and at laſt as faint as in *Fig.* 6.

EXPERIMENT VI. FIG. X.

Having made the Experiment as at *Fig.* 7, I preſſed another Priſm D F G, cloſe to the Priſm A B C, and when I ſqueezed them together but gently, ſome of

the Rays from the Candle R, paſſed through the lower Priſm, and falling upon a Paper at R, made a reddiſh Spot ; but when I ſqueezed them very hard, the Spot became much wider, white in the Middle, and only tinged with Red about the Edges: At the ſame Time the Eye ſaw a black Spot in the Image of the Candle at *k*; and a Stander-by looking obliquely at the Place I (where the Glaſſes touched) ſaw, as it were, a little Hole through the Priſms as big as the Spot *k*. But if the Priſms be preſſed together but gently, then all the other Phænomena diſappear, except the firſt little Spot at R, as in Fig. 11.

When the Candle is ſeen by Reflection from the lower Surface of a Priſm, as in the 7th, 9th and 10th Figures, the Rays paſs quite through that Surface, and are turned up again by the Attraction of it in Curve Lines ſo as to re-enter the Priſm, and then (going out again through the Surface A C) go up to the Eye at E. In this Caſe the moſt refrangible Rays, being the moſt eaſily inflected, make the leaſt Curves, whoſe Vertices are nearer the Glaſs than thoſe of the greater Curves made by the leaſt refrangible Rays. This is proved by Experiment 6, where the under Priſm only attracts down from the Reflection of the upper Priſm, the Red making Rays as in *Fig.* 11. where the Plate of Air between the Priſms is of ſome ſmall Thickneſs. But when the Priſms, whoſe Surfaces are a little convex, are preſſed hard together, the lower Priſm is near enough to attract Rays of a great Degree of Refrangibility; and therefore the Spot then becomes white in the Middle; and only red about the Edges, which are produced by ſuch Parts of the lower Priſm as are not ſo near the upper Priſm.

There

There are two Circumſtances in the 6th Experiment, which diſprove *Rizzetti*'s Aſſertion (Page 125) *viz.* That there is a ſenſible Reflection even where Glaſſes touch; for when the Priſms touch at I *Fig.* 10, the black Spot appearing in the Image of the Candle *k* ſhews that there is at I a Deficiency of thoſe Rays, which, coming from the Middle of the Candle, uſed to be reflected up to the Eye at E, and therefore that A B the reflecting Surface of the upper Priſm ceaſes to reflect in a little Space round about I where the upper Surface D F of the under Priſm touches it; the Rays, which before were reflected, now going down to make the Spot at R. The other Circumſtance is this; that whereas a Paper at *k* is inviſible to an Eye at E by the Interpoſition of the Priſm D F G; when another Priſm A C B is laid over it and preſſed hard, there appears to be an Hole of about $\frac{1}{8}$ of an Inch (more or leſs in Diameter as the priſmatical Surfaces are more or leſs flat) thro' which the Paper at *k* becomes viſible; this being the Place of Contact where the Reflection downwards (of the Surface D F) ceaſes.

This happens becauſe thoſe Rays, which (coming from the Candle K) were bent in Curves under the Surface A B of the upper Priſm about ſeveral Points near I, are by the Nearneſs of the Surface D F of the lower Priſm brought down to R, inſtead of being turned up again to the Eye at E; whilſt thoſe Rays, which (coming from the Paper at *k* thro' the Surface G F of the lower Priſm, and paſſing thro' the upper Surface of it F D) were bent in Curves about ſeveral Points near I, are prevented from turning down again to R, and are brought

brought up to the Eye at E, which conſequently muſt ſee a round Part of the Paper at *k*, juſt as big as the Place of Contact, which appears like an Hole ; or as if the two Priſms being changed to a Parallelopiped, were covered with a dark Paper that had only a ſmall Hole in it.

But to make this more evident, eſpecially to ſuch as are not well acquainted with Sir *Iſaac Newton*'s Opticks, I beg Leave to explain the Manner of the bending of the Rays where they are refracted or reflected.

Of the Bending of the Rays in their Refraction.

Let DD (Fig 12) repreſent a denſe Medium (as Glaſs) whoſe Surface is GG, and AA a rare Medium (as Air). Now let us ſuppoſe a Power to extend all over the Surface GG, acting from AA towards DD in Lines perpendicular to the Surface GG, very ſtrong in Contact, but inſenſible at a very ſmall Diſtance from the ſaid Surface, which we will call the *Attraction of the Surface* GG, without conſidering whether it be any real Virtue in the ſaid Surface, or the Action of a Medium impelling towards it. Let Lines 11, 22, 33, ſuch as expreſs the Lines in which the Attraction exerts it ſelf, and the Line MM (extremely near to GG) the Limits of the Attraction, beyond which it cannot affect a Ray of Light. Let the Ray of Light R *a* moving from a rare Medium into a Denſe in the Direction R r, come towards the Surface GG in ſuch an Angle that it may be refracted. When the Ray comes to *a*, by the Attraction at *a* it will be acted upon

in

in the Line *a* b, and (by the known Laws of Mechanicks) be turned out of the Way into the Direction *a* a, inſtead of *a* r: When it is got to *b*, being acted upon in the Direction *b* 4, its new Direction will become *b* b: At *c*, by the Power acting in the Line *c* 5, it will change its Direction to *c* c; and laſtly, at *d* it will go into the Glaſs in the Line *d* d, continuing in that ſtreight Line whilſt it moves in that Medium.

Now if the Lines 11, 22, 33, *n*, *c*, *b*, *a*, be infinitely near (as they muſt be ſuppoſed to be) the Ray, inſtead of being broken into the ſeveral ſtreight Lines *a b*, *b c*, and *c d*, will be bent into the Curve *a b c d*; and the emergent Ray *d* d will make the ſame Angle with the Incident Ray R r as if the Refraction had been made at once at the Point *n*, which Point may be conſidered as in the Surface G G, becauſe M M has been ſuppoſed extremely near that Surface: Then alſo may Refractions be conſidered in groſs, and Rays trac'd, in all Optical Propoſitions, as if there were no ſuch Curve as what we have been deſcribing.

Again, let D (*Fig.* 13) repreſent the denſe Medium or Glaſs, and A the rare Medium or Air; R *a* a Ray of Light coming out of the denſe Medium into the Rare, in the Direction R *r*, in which it may be refracted (as for Example, in an Angle of 30 Degrees with the Perpendicular P *a*). Let M M be the Line which limits the Attraction of the Surface G G, which Attraction is exerted in Lines tending perpendicularly from M M to G G. As ſoon as the Ray of Light has emerged at *a*, it is attracted in the Direction *a* P, and therefore diverted from the Line *a* r, into the new Direction *a* a; at *b*, it is turned into the Line *b* b; at *c*, into the Line *c* c; and at *d*, in-

d, into the Line *d* d; ſo that the emergent Ray will be *d* d, as if the Refraction had been performed in the Point *n*, and that Point was in the Surface G G, without any Curve at *a b c d*; and all the reſt as we conſidered it before, with this Difference only, *viz.* That the Ray is bent juſt as it comes out (or rather when it is come out) of the denſe Medium; whereas before we conſidered its Bending before it came into it.

Of the Bending of Rays in Reflection.

But if the Ray R *a* (*Fig.* 14) coming out of Glaſs into Air, ſhould come in ſuch a Direction as to be wholly reflected, as it will do when the Angle R *a* P is of 45 Degrees; I ſay the Reflection will not be made at the Surface G G, nor above it in the Glaſs; but under the ſaid Surface, in the Air, or even in a Vacuum, or any Medium leſs denſe, or rather leſs refractive than Glaſs.

M M repreſents the Limits of the Attraction of the Glaſs exerted in a Direction from M M to G G perpendicularly, as we ſaid before.

The Ray R *a*, moving in the Direction R r, at its Emerſion at *a*, is, for the Reaſons before given, turned into the Direction *a* a; then at *b*, into the Direction *b* b; at *c*, into the Direction *c* c; at *d*, into the Direction *d* d; at *e*, into the Direction *e* e; and at *f*, into the Direction *f* f parallel to G G; then at *g*, the Ray is again turned towards the Glaſs, by whoſe Attraction changing ſucceſſively into all the Directions *g* g, *i* i, *k* k, and *l* l; at laſt it re-enters the Glaſs in the Direction *m* m making

the

the ſame Angle with the Perpendicular *m* p that R *a* made with *a* P. Now as the Lines perpendicular to G G drawn from M M are infinitely near, the Line *a b c d e f g h i k l m* muſt be a Curve; and as M M and G G are extremely near, the Vertex of the Curve (whoſe Tangent is *f* f parallel to G G) will be ſo near the Point I, as to be conſidered as co-inciding with it, when we compare the Angle of Incidence with that of Reflection; then alſo will the Space between the Parallels p *m* and P *a* be ſo far diminiſhed, that thoſe two Lines may be looked upon as co-inciding, the Angles m *m* p and R *a* P being equal, whether the three Points *m*, I, *a*, co-incide or not.

For theſe Reaſons, for common Uſe, one may conſider the Reflection from the under Surface of the Glaſs as made at once in that Surface at the Point I. But when we examine Things ſtrictly, Experiments as well as the above Reaſoning, will ſhew, that there is ſuch a Curve as we have mentioned. *See* Experiment VI. *Fig.* 10 and 11; and we ſhall mention others.

N. B. If any Point of the Curve *a b c*, &c. between *a* and *f*, fall below (or beyond the Line M M, the Ray will then go on in a ſtraight Line Tangent to the Curve in that Point where it leaves the Line M M.

Now let us ſuppoſe M e d c b a r M (in the ſame *Fig.*) to be Glaſs, or any other denſe Medium, and m p P R Air, or any other rare Medium, and R *a* a Ray of Light moving in the rare Medium towards the denſe Medium in the Direction R *a* towards r; if inſtead of an Attraction at the Surface of the Glaſs

M M, there be ſuppoſed a repellent Force, whoſe Limits are G G ; then will the Ray by the Repulſion of the Surface M M be bent into the Curve *a b c d e f g h i k l m* in the ſame Manner as we ſhewed it would be under the Surface G G, when G p P G was conſidered as a denſe Medium. Hence it follows that a Ray moving in the Air, is reflected from a ſpecular Surface of Glaſs, or any other Mirrour, opake or diaphanous, without touching the ſaid Surface.

N. B. *That the ſame Power may, under different Circumſtances, attract to and repel from the ſame Surface, ſhall be made out in the remaining Part of this Paper ; but now taking ſuch a Power for granted, we will proceed in conſidering the Flexure of Rays of Light.*

Let us ſuppoſe a Priſm A C B (*Fig.* 15) to have the attracting Power of its inferior Surface extend as far as the Line *m m* ; if another Priſm G D F (the attracting Force of whoſe upper Surface extends as far as *n n*) be brought very near to the firſt Priſm ; where the attracting Powers of the Priſms interfere, they will deſtroy one another, becauſe they act in contrary Directions; and thereby the Limits of Attraction of each of the Surfaces will be contracted ; the Power of A B extending no farther than *n n*, and that of D F no farther than *m m*, whilſt the Space *n n m m* loſes all the Force that it had (and would have upon the Removal of either Priſm) to turn a Ray of Light, moving obliquely, out of its Direction.

Now in this Situation of the Priſms, a Ray of Light entering the Surface C B at right Angles, will go through the ſecond Priſm alſo at Right Angles

(not

(not exactly in the ſame Line, but) in a Line parallel to the Direction of the incident Ray: For Example, let the Ray *R a* (not refracted at, becauſe perpendicular to, the Surface C B) emerge from the firſt Priſm at *a*, in the Direction *a r*; its changed Direction at *a* will become *a* a, and at *b*, *b* b, or rather the Ray will be inflexed in the Curve *a b*; and at *b* getting out of the Power of the Attraction of the Surface A B, it will (for the Reaſons before given) move in a ſtraight Line from *b* to *c*, where it will be bent again the contrary Way in the Curve *c d* of the ſame kind as *a b*, and laſtly emerge in the Direction *d* d parallel to the firſt Direction R r. From hence it follows, that when the Priſms are brought ſo near as to touch, their mutual Attractions deſtroying each other, the Rays of Light will not be bent, but paſs through the two Priſms (which in this Caſe perform the Office of a Parallelopiped) in the ſame Direction with which they came into the firſt Priſm, and conſequently produce no Colours; contrary to what is affirmed by *Rizzetti* (*Page* 78, 79, &c.) and when the Rays R *a* fall obliquely upon the Surface C B, the Effect of their Refraction at their Immerſion at S to produce Colours, is taken off by the Refraction which they ſuffer at their Emerſion at z.

Experiment VII.

I took the Cube of *Fig.* 8, and looking obliquely through it at the Hole of the Window of my dark Chamber (the Sun ſhining or not ſhining) the Hole appeared entirely colourleſs, as did alſo a Candle,

both appearing fringed with Colours when ſeen through the Priſm. Then holding two Priſms together, as in *Fig.* 10, if the Hole of the dark Chamber be at k, it appears white to the Eye at E; but if the Angles of the Priſms at BF be a little ſeparated, whilſt the Points A D touch, the Hole will appear coloured: When the Surfaces are ſeparated at A D, and touch at B F, the Colours appear in an inverted Order; but if the Surfaces A D and BF are parallel, whether they touch or not, the Hole will appear white.

N. B. *In this Caſe the Priſms muſt be ſimilar, that the Surface* F G *may be parallel to* A C; *otherwiſe* A B *and* D F *muſt be ſo inclined to one another as to render* A C *and* F G *parallel. Indeed if one of the Priſms be very far removed from the other, the heterogeneous Light which entered in at* F G, *may be ſo far ſpread by the Separation of the differently refrangible Rays, that the Priſm* A B C *will not take it all in; then the Eye behind the ſecond Priſm may ſee Colours, as I ſuppoſe* Rizzeti *did.* See *Page* 79 of his Book.

If the Ray of Light R *a b c d* d (*Fig.* 15.) changing its Direction in the Manner above-mentioned, makes an Angle of about 45 Degres with the Perpendicular P *a*; upon the removal of the lower Priſm, the Ray will be turned up again, as in *Fig.* 14. But if the Angle P *a* R be greater, the Ray will ſtill be turned up again in a Curve, as *a b c d e f*, (*Fig.* 16) notwithſtanding the lower Priſm is at D F G; but if that Priſm be brought up cloſer to the Surface A B, the Curves will be deſtroyed where the Priſms touch, and all the Rays in the Place of Contact brought down through the lower Priſm.

THE

THE moſt refrangible Rays conſiſt of ſmaller Particles than the leaſt refrangible Rays, and therefore muſt have leaſt *Momentum*, the Velocity of all the Rays being the ſame; and conſequently are more eaſilyturned out of the Way by Attraction or Repulſion, which makes the Curves made by the purple and violet Rays under the Surface A B, to be leſs and nearer the ſaid Surface than the Curves made by red and orange Rays.

Suppoſe a Violet R *a* moving in the Direction R r (*Fig.* 16.) to be ſo bent under the Surface A B, that at the Vertex of the Curve, or where its Tangent *c c* is parallel to A B, there ſtill remains a ſmall Space between the Curve and the Line *n n*, where the Limits of Attraction (contracted by the Proximity of the undermoſt Priſm D F G end) that Ray will be turned up again in the Curve *d e f*, and ſo reflected in the Line *f* f, the Directions having been ſucceſſively changed, as in *Fig.* 14. But a red Ray with the ſame Inclination, would paſs on into the lower Priſm, as was explained in *Fig.* 15. Becauſe the Momentum of the red Ray being greater than that of the Violet, the ſame Degree of Attraction could not give it the ſame Flexure.

This is confirmed by Experiment, for when the lower Priſm is not preſſed hard againſt the upper (as in *Fig.* 11.) the Rays brought down to R make a Spot of a Colour made up chiefly of red and orange Rays; but when the Priſms are preſſed cloſer, the Spot grows bigger and perfectly white in its Middle, becauſe all Sorts of Rays are brought down to the Spot; but it is incloſed round with a reddiſh Border, occaſioned by the Parts of the Priſm which are very

near,

near, but not in Contact, or at leaſt not near enough to bring down the green, blue, purple and violet Rays. This ſhews that the Reflection is not made from the interior ſolid Parts of the Glaſs, nor from the Parts in the Surface, as *Rizzetti* affirms. But this is made more evident by

Experiment VIII. *Fig.* 17.

A Candle being in the Poſition K, the Eye at E, and the Priſm at ABC; a ſtrong Image of the Candle was ſeen at k as in *Fig.* 7. But lifting up a Veſſel of Water VSSV till the Surface of the Water VV touched AB the lower Surface of the Priſm, the Image of the Candle became almoſt inſenſible, as the Eye loſt all thoſe Rays which now were attracted into the Water. And for a farther Proof, that the Reflection is made under the Surface and not in it, when the Priſm was taken out of the Water, being wet at its lower Surface, or having a *Stratum* of Water (whoſe Surface was VV *Fig.* 18) under AB, the Image of the Candle did again become vivid, the Rays being turned up again under VV. Indeed the Image, in this Caſe, though ſtrong, did not appear well defined, by Reaſon of the Unevenneſs of the watry Surface VV *Fig.* 18.

I am very well aware that *Rizzetti* may anſwer here, that what I have ſaid above, does in ſome Meaſure favour his Notions; and that the Rays which (in *Fig.* 7, having paſſed through AB, the lower Surface of the Priſm) are turned up again to the Eye at E, do not ſuffer a Reflection but a new Immerſion; for he ſays, in *Page* 125. — "Anglus (meaning Sir

"*Iſaac*

" *Iſaac Newton*) ſecundo ſubjungit, *quod ſi lumen* " *in tranſitu è vitro in aerem obliquiùs incidat,* " *quam in angulo graduum* 40, *illud in totum re-* " *flectitur.* Ego verò reſpondeo, quod ex iis, quæ " docui in Prop. 4. Cap. 1. elicitur hanc non eſſe " veram luminis reflectionem, ſed potius novam Im- " merſionem; & ideo nego quod ex iſto Phænome- " no ſequatur lumen a partibus corporum ſolidis, " aliquo interjecto intervallo, reflecti." And a little lower, having quoted what Sir *Iſaac Newton* ſays, concerning the blue Light, which, coming from one Priſm obliquely upon the farther Surface of another, is wholly reflected, at the ſame Inclination that the red Light is wholly tranſmitted. — He ſays, " Satis " ſit iterum reſpondere, quod in hoc etiam caſu eſt " nova luminis immerſio, quæ dicitur ab auctore " reflectio.

But this is only cavilling about Words; for if the Ray of Light, which moving in a denſe Medium falls obliquely on the Surface common to that and a rarer Medium, be turned back again in the denſe Medium, ſo as to make the Angle in which it returns from the ſaid Surface equal to that in which it came to it; this Return of the Ray may properly be called a *Reflection*, whether the Ray be turned back at the Point of the Incidence in the Surface, or be carried about the Point of Incidence in a ſmall Curve, whoſe Conſideration may be omitted in tracing the Way of a Ray of Light in its Paſſage, for making of optical Machines. Whoever reads the 8th Prop. of the 2d Part, Book II. of Sir *Iſaac*'s Opticks, may very eaſily find that he was not ignorant of the turning back of the Ray under the Surface of the Glaſs be-

fore

fore it returned into it: And though the Reflection in that Caſe be not made by impinging on the ſolid Parts of the Glaſs, yet it is owing to them that the Light (acted upon at a Diſtance) is turned up again, as has been ſhewn by ſeveral of the Experiments abovementioned.

Now let us ſee how *Rizzetti*'s Account of the new Immerſion agrees with *Phænomena*.

Let all above the Line P p (*Fig.* 19) be a denſe Medium, as Glaſs; and all below it a rare Medium, as Air; A B C D is a Beam of Light inſenſible in Thickneſs, but of ſome Breadth, whoſe Rays cohere to one another, and whoſe Section or firſt Line is B C. If the Medium in which B C is, did not change, B C would move parallel to it ſelf in the Lines B *a* and C *d*; but as the End C of the Line B C comes out into a rare Medium, which being of leſs Reſiſtance to Light (for ſo he ſuppoſes) the Point C moving with more Facility than the Point B deſcribes the Curve C F H, whilſt B moving in the denſe Medium with more Difficulty, deſcribes the leſſer Curve B E G; then the Point C being got to H is re-immerſed, and the Line B C being got to H G goes on in the Direction H K G L parallel to itſelf, drawing the Beam after it in a rectilinear Direction, after Part of it has been bent within the Glaſs and Part of it without.

Now if this be true, and P p π be a Priſm, I beg to know what becomes of the Line at E F which unites the Rays of the Beam about the Point of Incidence I, when Water is brought to touch the Surface P p, as at A B *Fig.* 17? If it be ſaid that Water making a great Reſiſtance, though not ſo great as Glaſs, the Curve B E G deviates ſo little from the Line B *a* that the

the Point E comes below I, and the Beam is wholly refracted; I ask whence comes the faint Image at k? If it be anſwered, that ſome Part E I of the Line E F (*Fig.* 19) is turned up to the Eye at E (*Fig.* 17;) what becomes of the lateral Coheſion of Light on which *Rizzetti* founds his chief Propoſition, and from which he draws his Conſequences?

It would be tedious as well as uſeleſs to be particular in ſhewing all *Rizzetti*'s Miſtakes; therefore I ſhall only mention one more Experiment from Sir *Iſaac Newton*, which I repeated on Account of what is ſaid in *Rizzetti*'s Preface, *Page* 16, *viz. that if* (*according to Sir* Iſaac) *Rays were differently reflexible, Colours muſt be produced by Reflection from a plane Surface*; *but this*, ſays our Author, *is contrary to Experience.* Now this his Aſſertion is diſproved by

Experiment IX.

As this Experiment was made exactly in Sir *Iſaac Newton*'s Manner, and with the ſame Succeſs, I repeat the Account of it in his own Words.

" Let H F G [in Figure 20] repreſent a Priſm in " the open Air, and S the Eye of the Spectator, view- " ing the Clouds by their Light coming into the Priſm " at the plane Side F I G K, and reflected in it by its " Baſe H E I G, and thence going out through its plane " Side H E F K to the Eye: And when the Priſm and " Eye are conveniently placed, ſo that the Angles of " Incidence and Reflection at the Baſe may be of about " 40 Degrees, the Spectator will ſee a Bow M N, of a " blue Colour, running from one End of the Baſe to " the other, with the concave Side towards him, and

" the Part of the Base I M N G beyond this Bow will
" be brighter than the other Part E M N H on the o-
" ther Side of it. Now for understanding the Reason
" of it, suppose the Plane A B C to cut the plane Sides
" and the Base of the Prism perpendicularly. From
" the Eye to the Line B C wherein that Plane cuts the
" Base, draw the Lines S *p* and S *t* in the Angles S *p* C
" 50 Degrees $\frac{2}{10}$ and S *t* C 49 Degrees $\frac{1}{10}$, and the
" Point *p* will be the Limit beyond which none of
" the most refrangible Rays can pass through the
" Base of the Prism, and be refracted, whose Inci-
" dence is such that they may be reflected to the
" Eye; and the Point *t* will be the like Limit for
" the least refrangible Rays, that is, beyond which
" none of them can pass through the Base, whose In-
" cidence is such, that by Reflection they may come
" to the Eye. And the Point *r* taken in the middle
" Way between *p* and *t*, will be the like Limit for
" the meanly refrangible Rays. And therefore all
" the least refrangible Rays which fall upon the Base
" beyond *t*, that is, between *t* and B, and can come
" from thence to the Eye will be reflected thither:
" But on this Side *t*, that is, between *t* and C, many
" of these Rays will be transmitted through the Base:
" And all the most refrangible Rays which fall upon
" the Base beyond *p*, that is, between *p* and B and can
" by Reflection come from thence to the Eye, will be
" reflected thither, but every where between *p* and *t*,
" many of these Rays will get thro' the Base and be
" refracted; and the same is to be understood of the
" meanly refrangible Rays on either Side of the Point
" *r*. Whence it follows, that the Base of the Prism
" must every where between *t* and B by a total Re-

" flection of all Sorts of Rays to the Eye, look
" white and bright. And every where between *p*
" and C, by reaſon of the Tranſmiſſion of many
" Rays of either Sort, look more pale, obſcure and
" dark. But at *r*, and in other Places between *p*
" and *t*, where all the more refrangible Rays are re-
" flected to the Eye, and many of the leſs refrangi-
" gible are tranſmitted, the Exceſs of the moſt re-
" frangible in the reflected Light will tinge that Light
" with their Colour, which is violet and blue, this
" happens by taking the Line C *p r t* B any where
" between the Ends of the Priſm H G and E I.

If this needs any farther Explanation, let us ſuppoſe C A B the Section of the Priſm in *Fig.* 20. transferred to *Fig.* 21. at A C B. If R *o* be a red Ray inclined to a Perpendicular to A B in an Angle of more than 41 or 42 Degrees, it will at its Emerſion under the Surface A B be turned into the Curve *o n m i*, and ſo go up again to the Eye at E; but another red Ray coming in the Direction *r n* making an Angle with the Perpendicular ſufficiently leſs, will after its Emerſion at *n*, be only bent ſo much as to be turned out of the Way, and refracted to *q*, in an Angle of Refraction agreeable to the Refrangibility of red Light. But V *m* a violet Ray with the ſame Inclination as the laſt red one *r n* ſhall not be refracted, but turned up in the Curve *m i* P, and ſo go to the Eye at E. Another violet Ray v *m* making an Angle ſomething leſs with the Perpendicular, will paſs through the Glaſs, and be refracted in the Line *m* S. Upon this Account all that Part of the Baſe of the Priſm (of which A B is the Section) between A and *p* will be dark or faint, all that Part between *p*

and *n* be tinged with a bluiſh Colour, and all between *o* and B of a bright White.

POSTSCRIPT.

THE Bending of Rays of Light juſt as they come to be reflected or refracted, may be eaſily underſtood by ſuch as are well acquainted with thoſe Properties of Light, which Sir *Iſaac Newton* calls their *Fits of eaſy Reflection*, and *Fits of eaſy Tranſmiſſion*; without any Hypotheſis, but by Conſequences fairly drawn from Experiments and Obſervations. But as Signior *Rizzetti* does not ſeem (in his Book) to have the leaſt Notion of thoſe Properties of Light, and the nice Obſervations on which they are founded; and ſeveral other Perſons have not Time to read thoſe Parts of the Opticks with ſufficient Application; to ſhew how the ſame Power of the Surface of a denſe Medium may both attract and repel under different Circumſtances —— I content myſelf here with giving the Hypotheſis, which Sir *Iſaac* does before he comes to that Part of his Book where he demonſtrates the Fits abovementioned.

If G G be the Surface of a denſe Medium G D D G, on which a Tremor is cauſed by the Warmth communicated to it by the Rays of Light, ſo as to give a Wave-like Motion to the Medium immediately next to the Surface G G; as that vibratory Motion is performed, the Medium alternately puſhes from the Surface, and returns towards it (as is repreſented by the Poſition of the Darts in the

the Darts in Figure) and puſhes back the Light ſo as to reflect it when the Vibration is contrary to its Direction, but brings it down to be refracted when the Vibration conſpires with the ſaid Motion. *See a further Account of this in Sir* Iſaac Newton's *Opticks*, Book II. Part 3. Propoſition 12.

The Perſons preſent at the Experiments above-mentioned, tried them as well as myſelf, and being ſatisfied with the Succeſs of them, allowed me to mention it, and make uſe of their Names in this Account.

Of the ROYAL SOCIETY.

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Dr. *Scheutzer*.
Mr. *Grey*.
Mr. *Georges*.
Mr. *Dugood*.

Other GENTLEMEN.

Colonel *Spotſwood*.
Mr. *Haily*.
Mr. *Graham*.
Mr. *Hewet*.

FOREIGNERS.

The Abbot *Lercari*, a Nobleman of *Genoa*, Couſin to Cardinal *Lercari*.
The Abbot *Cuzzoni*.
The Abbot *Rolli*, and his Brother.

III. *The*

III. *The Method of making* Tin-Plates, *extracted from the Memoirs of the Academy of Sciences, for the Year* 1725, *by* William Rutty, *M. D. R. S. Secr.*

THE making of Tin-plates, or Lattin, as it is called, being not commonly practiſed in *England*, though there is ſo great a Conſumption of it, either becauſe the Method is not ſufficiently known, or becauſe that in Uſe to make ſmall Quantities for particular Purpoſes is much too dear to anſwer the Artificer's Expectation in making larger, whereby we are obliged to export our own Tin to *Germany*, to receive it back again *manufactured*, I thought it not improper to lay before the Society the Method the *Germans* themſelves make uſe of, as I have extracted it from a Diſſertation of Mr. *De Reaumur*, printed in the laſt Volume of the Memoirs of the Academy of Sciences of *Paris*, in which alſo he lays down ſome Improvements, as he thinks, of his own.

He takes notice then that the making of Tin-plates, (which is called in *France*, *white Iron*) does not properly begin, till they go about to prepare the Leaves or Plates of Iron that are to be tinned, which are ſuppoſed to be ſufficiently thin and flat, and cut into Squares: But there are only certain Sorts of Iron which can be reduced into theſe Leaves, of which thoſe are the moſt proper, that when heated are eaſieſt extendible, and yet can be forged with a Hammer when cold; the more ſoft and extremely flexible, as well

well as the more brittle being to be rejected. Theſe Leaves are drawn from Bars of Iron, about an Inch ſquare; which being made a little flat, they cut into thin Pieces or Soles (*ſemelles*) which they fold together, and having made them into Parcels containing forty Leaves each, beat them all at once with a Hammer that weighs from 600 to 700 ℔ Weight. After this, the principal Part of the whole Art is to prepare theſe Leaves; for the lighteſt Duſt, or the leaſt Ruſt upon their Surface will prevent the Tin from uniting with them. This may indeed be taken off by filing, but that being much too expenſive, the ſame may be brought about by ſteeping the Plates in acid Waters, for a certain Time, to what Number they pleaſe, and when they are taken out, ſcouring them with Sand, in order to fetch off any Thing that may remain upon the Surface: And by this Method a Woman may clean more Plates in an Hour, than the moſt expeditious Workman can file in many Days. Of theſe Waters the Author mentions ſeveral; but what the *Germans* themſelves uſed, and which they make a mighty Secret of, he found to be only common Water made eager with Rye, which requires very little Pains. For after they have ground the Grain groſly, and pounded it, they leave it to ferment in common Water for a certain Time, and with a little Patience they are ſure to have an eager Menſtruum. With this Menſtruum they fill Troughs or Tuns, into which they put Piles of Iron Plates; and to make it grow eager the better, and to have more Activity, they keep theſe Veſſels in Vaults or Stoves which have little Air, and in which they keep lighted Charcoal. The Workmen go into theſe Vaults once or twice in a Day, either to turn the

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Plates

Plates that they may be equally expoſed to the Action of the acid Liquor, or to take out thoſe that are ſufficiently cleanſed, or to put others in their room: And as the Liquor is more acid, or the Heat of the Vault or Stove is more intenſe, the Plates are ſooner cleanſed; but it requires at leaſt two Days, and ſometimes a great deal more. This is the Method which the *Germans* employed in the Tin-works in *France*, conſtantly made uſe of to prepare the Iron-plates to receive the Coat of Tin: But as the Author obſerved, that the conſtant Attendance upon them in the Stoves was very laborious, the Heat therein being almoſt inſupportable to thoſe who are not uſed to it, he propoſes ſome other Methods which are attended with very little Trouble, and as ſmall, if not a leſs Expence; and which upon Trial ſucceeded full as well. Having therefore obſerved that the Iron-leaves or Plates are covered with a Scale or Layer, half vitrified by the Fire, on which Acids have none or very little Effect, he imagined that inſtead of *diſſolving* the Iron in theſe acid Waters, it would be better to make it *ruſt*, and thereby put it in a Condition to be eaſier cleanſed from theſe Scales; as Ruſt is accompanied with a ſort of Fermentation and Rarefaction, and the Matter which ruſts takes up a greater Space, and raiſes up whatever oppoſes it. To this Purpoſe he ſteeped Iron Plates in different eager Menſtruums, as in Water in which Alum, common Salt and Sal-armoniack were ſeparately diſſolved; and others of the ſame Iron he only dipped into the ſame Waters, and inſtantly taking them out expoſed them to the Air. Theſe latter were ruſted by all of them, but ſooner by that in which the Sal-armoniack was diſſolved. After two Days, during which every Plate had been dipped

ped into the Menſtruum but twice or thrice, he ſcoured them, and likewiſe thoſe he had left to ſteep for that Time; and comparing them together, found that thoſe, which had been only wetted at different Times, cleanſed better than thoſe which were ſteeped; the Ruſt covering all the Surface of the latter without raiſing the Scale; whereas in the former, as ſoon as one Part of the Metal is detached, it is attracted by the Menſtruum, and the Surface is raiſed into Bliſters of Ruſt. Theſe Diſſolvents, the Author takes notice, tho' weak in themſelves, yet produce the Effect as well as the ſtronger, which are much dearer: But amongſt the latter he prefers Vinegar, which being very plentiful in *France*, may be uſed with little Coſt. For you need only dip each Leaf into it, and take it out again immediately, leaving it afterwards in ſome moiſt Place, and it will be ſcaled in eight and forty Hours, if you take care to repeat this 3 or 4 Times in a Day. The ſcaling will ſtill be more expeditious, if you diſſolve a little Sal-armoniack in the Vinegar, a Pound or two to a Puncheon; for as the Vinegar diſſolves Iron well, ſo Sal-armoniack, as juſt obſerved, ruſts it ſooner than any other Salt: But this muſt be uſed very moderately, and the Leaf muſt be left to ſteep in clean Water to diſſolve any Particles of it that may ſtick to its Surface, which may otherwiſe make it ruſt after it is tinned. If you ſcale with Vinegar, and want to do it at a leſs Expence, you need only plunge the Leaves once or twice at fartheſt, and when the Vinegar is dried upon the Surface, ſprinkle it with Water; or dip them into it, and take them out immediately. There are ſeveral other Ways of making Iron ruſt, as keeping it in a moiſt Cellar, expoſing it to the Dew, ſprinkling

it with ſimple Water, ſeveral Times in a Day, which will ſtill act quicker by diſſolving Sal-armoniack in it. In thoſe Countries where the Pyrites is common, the Vitriolick Waters will ſcale them ſoon enough, which are almoſt as cheap as common Water: You need only heap the Pyrites together, and leaving them to moulder in the Air, make afterwards a Lixivium with them and common Water, which Lie will have the deſired Effect: But as the Leaves of Iron are ſenſibly much eaſier cleanſed on one Side than the other, the bad Side rarely taking the brilliant Poliſh in the tinning, but having always ſome Spots, which proceeds in that in the beating one Side is more expoſed to the Action of the Hammer, and is therefore better plained, the Author again adviſes not to ſteep them, but only to moiſten them, in order to make them ruſt, whereby you need moiſten that Side only that wants it moſt: Whereas if you ſteep them, as the bad Side will take double or triple the Time of the other, the acid Menſtruum will diſſolve the Surface, and occaſion a Loſs of Iron. He next gives two Cautions neceſſary to be followed: the firſt is in the Management of the Plates before they come to be prepared; which is in the beating of them, to change the Place of each in its Turn, that every one may receive the immediate Action of the Hammer, otherwiſe they will not extend equally: the ſecond is to ſteep them in Clay or Fuller's earth tempered with Water before you heat them, to prevent their ſoldering with one another. He then cloſes this Part of the Operation with remarking that whatſoever of theſe Methods are pitched upon, whether the old one, of which he has learnt the Secret, or any of the new, which he has here ſhewn, it is abſolutely neceſſary after the Plates

are ſufficiently ſcaled, to ſcour them with Sand, and when there remains no more black Spots upon their Surface, to throw them into Water to prevent their ruſting again, and leave them in it till the Inſtant you would tin them, or in the Term of Art, blanch them. This he obſerves is the very Object of the whole Art, and is kept as much a Secret by the *Blancher*, as the acid eroding Menſtruum is by the *Scaler*: But the Manner of doing it is thus. They flux the Tin in a large iron Crucible, which has the Figure of a broken Pyramid with four Faces, of which the two oppoſite ones are leſs than the two others. This Crucible they heat only from below, its upper Border being luted in the Furnace quite round. The Crucible is always deeper than the Plates which are to be tinned are long, which they always put in downright, and the Tin ought to ſwim over them. For this Purpoſe Artificers of different Trades prepare the Plates in different Manners, which are all exceptionable: But the *Germans* he perceived made uſe of no Preparation whatſoever, except putting the ſcoured Plates into clean Water, as juſt remarked; but when the Tin is melted in the Crucible, they cover it with a Layer of a Sort of Suet, an Inch or two thick, through which the Plate muſt paſs before it comes to the Tin: The firſt Uſe of which is to keep the Tin from burning, and if any Part ſhould take Fire, as the Suet will ſoon moiſten it, to reduce it to its natural State again. This Suet is compounded, as the Blanchers ſay, and is of a black Colour, which the Author thought might be given it with Soot or the Smoak of a Chimney, only to ſpread a Myſtery over their Work; but he found it true ſo far, that common unprepared Suet was not ſufficient: For after ſeveral Attempts, there

was always ſomething wanting to render the Succeſs of the Operation certain. The whole Secret then of Blanching lies entirely in the Preparation of this Suet: And this he at laſt diſcovered to conſiſt only in firſt frying and burning it; which not only gives it the Colour, but puts it into a Condition to give the Iron a Diſpoſition to be tinned, which it does ſurpriſingly. The Tin itſelf ought to have a certain Degree of Heat; for if it is not hot enough it will not ſtick to the Iron; if it is too hot, it will cover it with too thin a Coat, and the Plates will have ſeveral Colours, as a Mixture of red, blue, and yellow, and the whole appear of a villainous yellow Caſt. To prevent this, by knowing when the Tin has a proper Degree of Heat, they might firſt make an Eſſay with ſmall Pieces of the ſcaled Plates, and they would learn from them when the Tin is in proper Order: But generally ſpeaking, they dip the Plates into Tin that is more or leſs hot, according to the Thickneſs they would have the Coat to be of. Some Plates they only give one Layer to, and theſe they plunge into Tin, that has a leſſer Degree of Heat than that into which they plunge thoſe Plates which they would have take two Layers; as alſo when they give theſe the ſecond Layer, they put them into Tin that has not ſo great a Degree of Heat, as that into which they were put the firſt Time: Beſides which, it is to be obſerved, that the Tin, which is to give the ſecond Coat, ought to be freſh covered with Suet, but only with the common Sort without Preparation; for melted Tin is ſufficiently diſpoſed to attach it ſelf to ſolid Tin; and in this Caſe it is to tin itſelf, to which the new Tin is to be joined. As to the Choice of the Tin, the Manner of making it is as bright as poſſible, with a Num-

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ber of little Articles neceſſary to the Practice, the Author refers them to another Time, as more properly belonging to the Deſcription of the whole Art, than to a Memoir in which he only gives the Principles of it.

IV. *A Letter from the Reverend Mr.* James Bradley *Savilian Profeſſor of Aſtronomy at* Oxford, *and F.R.S. to Dr.* Edmond Halley Aſtronom. Reg. &c. *giving an Account of a new diſcovered Motion of the* Fix'd Stars.

SIR,

YOU having been pleaſed to expreſs your Satisfaction with what I had an Opportunity ſometime ago, of telling you in Converſation, concerning ſome Obſervations, that were making by our late worthy and ingenious Friend, the honourable *Samuel Molyneux* Eſquire, and which have ſince been continued and repeated by my ſelf, in order to determine the *Parallax* of the *fixt Stars* ; I ſhall now beg leave to lay before you a more particular Account of them.

Before I proceed to give you the Hiſtory of the Obſervations themſelves, it may be proper to let you know, that they were at firſt begun in hopes of verifying and confirming thoſe, that Dr. *Hook* formerly communicated to the publick, which ſeemed to be attended with Circumſtances that promiſed greater Exactneſs in them, than could be expected in any other, that had been made and publiſhed on the ſame Account. And as his Attempt was what principally gave Riſe to this, ſo his Method in making the Obſervations was in ſome Mea-

Meaſure that which Mr. *Molyneux* followed: For he made Choice of the ſame Star, and his Inſtrument was conſtructed upon almoſt the ſame Principles. But if it had not greatly exceeded the Doctor's in Exactneſs, we might yet have remained in great Uncertainty as to the *Parallax* of the *fixt Stars*; as you will perceive upon the Compariſon of the two Experiments.

This indeed was chiefly owing to our curious Member, Mr. *George Graham*, to whom the Lovers of Aſtronomy are alſo not a little indebted for ſeveral other exact and well-contrived Inſtruments. The Neceſſity of ſuch will ſcarce be diſputed by thoſe that have had any Experience in making Aſtronomical Obſervations; and the Inconſiſtency, which is to be met with among different Authors in their Attempts to determine ſmall Angles, particularly the annual Parallax of the *fixt Stars*, may be a ſufficient Proof of it to others. Their Diſagreement indeed in this Article, is not now ſo much to be wondered at, ſince I doubt not, but it will appear very probable, that the Inſtruments commonly made uſe of by them, were liable to greater Errors than many times that Parallax will amount to.

The Succeſs then of this Experiment evidently depending very much on the Accurateneſs of the Inſtrument that was principally to be taken Care of: In what Manner this was done, is not my preſent Purpoſe to tell you; but if from the Reſult of the Obſervations which I now ſend you, it ſhall be judged neceſſary to communicate to the Curious the Manner of making them, I may hereafter perhaps give them a particular Deſcription, not only of Mr. *Molyneux*'s Inſtrument, but alſo of my own,

which

which hath ſince been erected for the ſame Purpoſe and upon the like Principles, though it is ſomewhat different in its Conſtruction, for a Reaſon you will meet with preſently.

Mr. *Molyneux*'s *Apparatus* was compleated and fitted for obſerving about the End of *November* 1725, and on the third Day of *December* following, the bright Star in the Head of *Draco* (marked γ by *Bayer*) was for the firſt Time obſerved, as it paſſed near the Zenith, and its Situation carefully taken with the Inſtrument. The like Obſervations were made on the 5th, 11th, and 12th Days of the ſame Month, and there appearing no material Difference in the Place of the Star, a farther Repetition of them at this Seaſon ſeemed needleſs, it being a Part of the Year, wherein no ſenſible Alteration of Parallax in this Star could ſoon be expected. It was chiefly therefore Curioſity that tempted me (being then at *Kew*, where the Inſtrument was fixed) to prepare for obſerving the Star on *December* 17th, when having adjuſted the Inſtrument as uſual, I perceived that it paſſed a little more Southerly this Day than when it was obſerved before. Not ſuſpecting any other Cauſe of this Appearance, we firſt concluded, that it was owing to the Uncertainty of the Obſervations, and that either this or the foregoing were not ſo exact as we had before ſuppoſed; for which Reaſon we purpoſed to repeat the Obſervation again, in order to determine from whence this Difference proceeded; and upon doing it on *December* 20th, I found that the Star paſſed ſtill more Southerly than in the former Obſervations. This ſenſible Alteration the more ſurprized us, in that it was the contrary

way

way from what it would have been, had it proceeded from an annual *Parallax* of the *Star*: But being now pretty well ſatisfied, that it could not be entirely owing to the want of Exactneſs in the Obſervations; and having no Notion of any thing elſe, that could cauſe ſuch an apparent Motion as this in the Star; we began to think that ſome Change in the Materials, &c. of the Inſtrument itſelf, might have occaſioned it. Under theſe Apprehenſions we remained ſome time, but being at length fully convinced, by ſeveral Trials, of the great Exactneſs of the Inſtrument, and finding by the gradual Increaſe of the Stars Diſtance from the Pole, that there muſt be ſome regular Cauſe that produced it; we took care to examine nicely, at the Time of each Obſervation, how much it was: and about the Beginning of *March* 1726, the Star was found to be 20″ more Southerly than at the Time of the firſt Obſervation. It now indeed ſeemed to have arrived at its utmoſt Limit Southward, becauſe in ſeveral Trials made about this Time, no ſenſible Difference was obſerved in its Situation. By the Middle of *April* it appeared to be returning back again towards the North; and about the Beginning of *June*, it paſſed at the ſame Diſtance from the Zenith as it had done in *December*, when it was firſt obſerved.

From the quick Alteration of this Star's Declination about this Time (it increaſing a Second in three Days) it was concluded, that it would now proceed Northward, as it before had gone Southward of its preſent Situation; and it happened as was conjectured: for the Star continued to move Northward till *September* following, when it again became ſtationary,

tionary, being then near 20″ more Northerly than in *June*, and no leſs than 39″ more Northerly than it was in *March*. From *September* the Star returned towards the South, till it arrived in *December* to the ſame Situation it was in at that time twelve Months, allowing for the Difference of Declination on account of the Preceſſion of the Equinox.

This was a ſufficient Proof, that the Inſtrument had not been the Cauſe of this apparent Motion of the Star, and to find one adequate to ſuch an Effect ſeemed a Difficulty. A Nutation of the Earth's Axis was one of the firſt things that offered itſelf upon this Occaſion, but it was ſoon found to be inſufficient; for though it might have accounted for the change of Declination in γ *Draconis* yet it would not at the ſame time agree with the Phænomena in other Stars; particularly in a ſmall one almoſt oppoſite in right Aſcenſion to γ *Draconis*, at about the ſame Diſtance from the North Pole of the Equator: For, though this Star ſeemed to move the ſame way, as a Nutation of the Earth's Axis would have made it, yet it changing its Declination but about half as much as γ *Draconis* in the ſame time (as appeared upon comparing the Obſervations of both made upon the ſame Days, at different Seaſons of the Year) this plainly proved, that the apparent Motion of the Stars was not occaſioned by a real Nutation, ſince if that had been the Cauſe, the Alteration in both Stars would have been near equal.

The great Regularity of the Obſervations left no room to doubt, but that there was ſome regular Cauſe that produced this unexpected Motion, which did not depend on the Uncertainty or Variety of the

Seaſons of the Year. Upon comparing the Obſervations with each other, it was diſcovered, that in both the fore-mentioned Stars, the apparent Difference of Declination from the *Maxima*, was always nearly proportional to the verſed Sine of the Sun's Diſtance from the Equinoctial Points. This was an Inducement to think, that the Cauſe, whatever it was, had ſome Relation to the Sun's Situation with reſpect to thoſe Points. But not being able to frame any Hypotheſis at that Time, ſufficient to ſolve all the Phænomena, and being very deſirous to ſearch a little farther into this Matter; I began to think of erecting an Inſtrument for my ſelf at *Wanſted*, that having it always at Hand, I might with the more Eaſe and Certainty, enquire into the Laws of this new Motion. The Conſideration likewiſe of being able by another Inſtrument, to confirm the Truth of the Obſervations hitherto made with Mr. *Molyneux*'s, was no ſmall Inducement to me; but the Chief of all was, the Opportunity I ſhould thereby have of trying, in what Manner other Stars were affected by the ſame Cauſe, whatever it was. For Mr. *Molyneux*'s Inſtrument being originally deſigned for obſerving γ *Draconis* (in order, as I ſaid before, to try whether it had any ſenſible Parallax) was ſo contrived, as to be capable of but little Alteration in its Direction, not above ſeven or eight Minutes of a Degree: and there being few Stars within half that Diſtance from the Zenith of *Kew*, bright enough to be well obſerved, he could not, with his Inſtrument, throughly examine how this Cauſe affected Stars differently ſituated with

reſpect

respect to the equinoctial and solstitial Points of the Ecliptick.

These Considerations determined me; and by the Contrivance and Direction of the same ingenious Person, Mr. *Graham*, my Instrument was fixed up *August* 19, 1727. As I had no convenient Place where I could make use of so long a Telescope as Mr. *Molyneux*'s, I contented my self with one of but little more than half the Length of his (*viz.* of about $12\frac{3}{4}$ Feet, his being $24\frac{1}{4}$) judging from the Experience which I had already had, that this Radius would be long enough to adjust the Instrument to a sufficient Degree of Exactness, and I have had no Reason since to change my Opinion: for from all the Trials I have yet made, I am very well satisfied, that when it is carefully rectified, its Situation may be securely depended upon to half a Second. As the Place where my Instrument was to be hung, in some Measure determined its Radius, so did it also the Length of the Arch, or Limb, on which the Divisions were made to adjust it: For the Arch could not conveniently be extended farther, than to reach to about $6\frac{1}{4}^{\circ}$ on each Side my Zenith. This indeed was sufficient, since it gave me an Opportunity of making Choice of several Stars, very different both in Magnitude and Situation; there being more than two hundred inserted in the *British* Catalogue, that may be observed with it. I needed not to have extended the Limb so far, but that I was willing to take in *Capella*, the only Star of the first Magnitude that comes so near my Zenith.

My Instrument being fixed, I immediately began to observe such Stars as I judged most proper to

give me light into the Cauſe of the Motion already mentioned. There was Variety enough of ſmall ones; and not leſs than twelve, that I could obſerve through all the Seaſons of the Year; they being bright enough to be ſeen in the Day-time, when neareſt the Sun. I had not been long obſerving, before I perceived, that the Notion we had before entertained of the Stars being fartheſt North and South, when the Sun was about the Equinoxes, was only true of thoſe that were near the ſolſtitial Colure: And after I had continued my Obſervations a few Months, I diſcovered, what I then apprehended to be a general Law, obſerved by all the Stars, *viz.* That each of them became ſtationary, or was fartheſt North or South, when they paſſed over my Zenith at ſix of the Clock, either in the Morning or Evening. I perceived likewiſe, that whatever Situation the Stars were in with reſpect to the cardinal Points of the Ecliptick, the apparent Motion of every one tended the ſame Way, when they paſſed my Inſtrument about the ſame Hour of the Day or Night; for they all moved Southward, while they paſſed in the Day, and Northward in the Night; ſo that each was fartheſt North, when it came about Six of the Clock in the Evening, and fartheſt South, when it came about Six in the Morning.

Though I have ſince diſcovered, that the *Maxima* in moſt of theſe Stars do not happen exactly when they come to my Inſtrument at thoſe Hours, yet not being able at that time to prove the contrary, and ſuppoſing that they did, I endeavoured to find out what Proportion the greateſt Alterations of Declination in different Stars bore to each other; it being

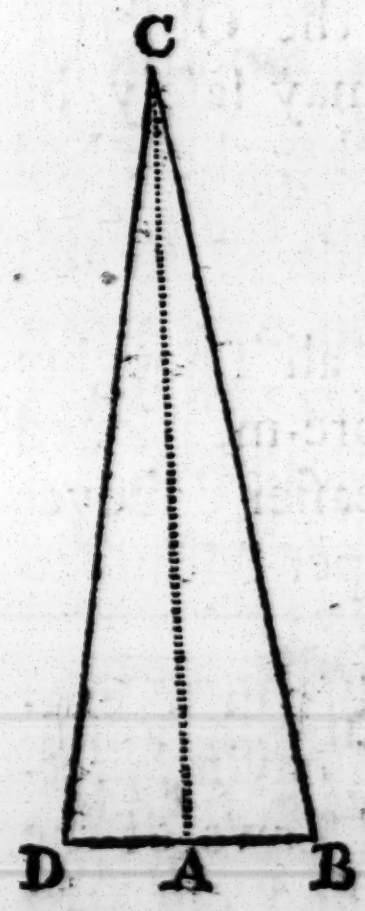
very

very evident, that they did not all change their Declination equally. I have before taken notice, that it appeared from Mr. *Molyneaux*'s Obſervations, that γ *Draconis* altered its Declination about twice as much as the fore-mentioned ſmall Star almoſt oppoſite to it; but examining the matter more particularly, I found that the greateſt Alteration of Declination in theſe Stars, was as the Sine of the Latitude of each reſpectively. This made me ſuſpect that there might be the like Proportion between the *Maxima* of other Stars; but finding, that the Obſervations of ſome of them would not perfectly correſpond with ſuch an Hypotheſis, and not knowing, whether the ſmall Difference I met with, might not be owing to the Uncertainty and Error of the Obſervations, I deferred the farther Examination into the Truth of this Hypotheſis, till I ſhould be furniſhed with a Series of Obſervations made in all Parts of the Year; which might enable me, not only to determine what Errors the Obſervations are liable to, or how far they may ſafely be depended upon; but alſo to judge, whether there had been any ſenſible Change in the Parts of the Inſtrument itſelf.

Upon theſe Conſiderations, I laid aſide all Thoughts at that Time about the Cauſe of the fore-mentioned Phænomena, hoping that I ſhould the eaſier diſcover it, when I was better provided with proper Means to determine more preciſely what they were.

When the Year was compleated, I began to examine and compare my Obſervations, and having pretty well ſatisfied my ſelf as to the general Laws of the *Phænomena*, I then endeavoured to find out the Cauſe

Cauſe of them. I was already convinced, that the apparent Motion of the Stars was not owing to a Nutation of the Earth's Axis. The next Thing that offered itſelf, was an Alteration in the Direction of the Plumb-line, with which the Inſtrument was conſtantly rectified; but this upon Trial proved inſufficient. Then I conſidered what Refraction might do, but here alſo nothing ſatisfactory occurred. At laſt I conjectured, that all the *Phænomena* hitherto mentioned, proceeded from the progreſſive Motion of Light and the Earth's annual Motion in its Orbit. For I perceived, that, if Light was propagated in Time, the apparent Place of a fixt Object would not be the ſame when the Eye is at Reſt, as when it is moving in any other Direction, than that of the Line paſſing through the Eye and Object; and that, when the Eye is moving in different Directions, the apparent Place of the Object would be different.



I conſidered this Matter in the following Manner. I imagined C A to be a Ray of Light, falling perpendicularly upon the Line B D; then if the Eye is at reſt at A, the Object muſt appear in the Direction A C, whether Light be propagated in Time or in an Inſtant. But if the Eye is moving from B towards A, and Light is propagated in Time, with a Velocity that is to the Velocity of the Eye, as C A to B A; then Light moving from C to A, whilſt the Eye moves from B to A, that Particle of it,

it, by which the Object will be diſcerned, when the Eye in its Motion comes to A, is at C when the Eye is at B. Joining the Points B, C, I ſuppoſed the Line CB, to be a Tube (inclined to the Line BD in the Angle DBC) of ſuch a Diameter, as to admit of but one Particle of Light; then it was eaſy to conceive, that the Particle of Light at C (by which the Object muſt be ſeen when the Eye, as it moves along, arrives at A) would paſs through the Tube BC, if it is inclined to BD in the Angle DBC, and accompanies the Eye in its Motion from B to A; and that it could not come to the Eye, placed behind ſuch a Tube, if it had any other Inclination to the Line BD. If inſtead of ſuppoſing CB ſo ſmall a Tube, we imagine it to be the Axis of a larger; then for the ſame Reaſon, the Particle of Light at C, could not paſs through that Axis, unleſs it is inclined to BD, in the Angle CBD. In like manner, if the Eye moved the contrary way, from D towards A, with the ſame Velocity; then the Tube muſt be inclined in the Angle BDC. Although therefore the true or real Place of an Object is perpendicular to the Line in which the Eye is moving, yet the viſible Place will not be ſo, ſince that, no doubt, muſt be in the Direction of the Tube; but the Difference between the true and apparent Place will be (*cæteris paribus*) greater or leſs, according to the different Proportion between the Velocity of Light and that of the Eye. So that if we could ſuppoſe that Light was propagated in an Inſtant, then there would be no Difference between the real and viſible Place of an Object, altho' the Eye were in Motion, for in that caſe, AC being infinite with Reſpect to AB, the Angle ACB (the

Difference between the true and visible Place) vanishes. But if Light be propagated in Time (which I presume will readily be allowed by most of the Philosophers of this Age) then it is evident from the foregoing Considerations, that there will be always a Difference between the real and visible Place of an Object, unless the Eye is moving either directly towards or from the Object. And in all Cases, the Sine of the Difference between the real and visible Place of the Object, will be to the Sine of the visible Inclination of the Object to the Line in which the Eye is moving, as the Velocity of the Eye to the Velocity of Light.

If Light moved but 1000 times faster than the Eye, and an Object (supposed to be at an infinite Distance) was really placed perpendicularly over the Plain in which the Eye is moving, it follows from what hath been already said, that the apparent Place of such an Object will be always inclined to that Plain, in an Angle of $89^{\circ}\ 56'\frac{1}{2}$; so that it will constantly appear $3'\frac{1}{2}$ from its true Place, and seem so much less inclined to the Plain, that way towards which the Eye tends. That is, if A C is to A B (or A D) as 1000 to one, the Angle A B C will be $89^{\circ}\ 56'\frac{1}{2}$, and A C B $= 3'\frac{1}{2}$, and B C D $=$ 2 A C B $= 7'$. So that according to this Suppositio^{n}, the visible or apparent Place of the Object will be altered $7'$, if the Direction of the Eye's Motion is at one time contrary to what it is at another.

If the Earth revolve round the Sun annually, and the Velocity of Light were to the Velocity of the Earth's Motion in its Orbit (which I will at present suppose to be a Circle) as 1000 to one; then tis easy

to

to conceive, that a Star really placed in the very Pole of the Ecliptick, would, to an Eye carried along with the Earth, ſeem to change its Place continually, and (neglecting the ſmall Difference on the Account of the Earth's diurnal Revolution on its Axis) would ſeem to deſcribe a Circle round that Pole, every Way diſtant therefrom $3' \frac{5}{7}$. So that its Longitude would be varied through all the Points of the Ecliptick every Year; but its Latitude would always remain the ſame. Its right Aſcenſion would alſo change, and its Declination, according to the different Situation of the Sun in reſpect to the equinoctial Points; and its apparent Diſtance from the North Pole of the Equator would be $7'$ leſs at the Autumnal, than at the vernal Equinox.

The greateſt Alteration of the Place of a Star in the Pole of the Ecliptick (or which in Effect amounts to the ſame, the Proportion between the Velocity of Light and the Earth's Motion in its Orbit) being known; it will not be difficult to find what would be the Difference upon this Account, between the true and apparent Place of any other Star at any time; and on the contrary, the Difference between the true and apparent Place being given; the Proportion between the Velocity of Light and the Earth's Motion in its Orbit may be found.

As I only obſerved the apparent Difference of Declination of the Stars, I ſhall not now take any farther Notice in what manner ſuch a Cauſe as I have here ſuppoſed would occaſion an Alteration in their apparent Places in other Reſpects; but, ſuppoſing the Earth to move equally in a Circle, it may be gathered from what hath been already ſaid, that a Star which

is neither in the Pole nor Plain of the Ecliptick, will ſeem to deſcribe about its true Place a Figure, inſenſibly different from an Ellipſe, whoſe Tranſverſe Axis is at Right-angle to the Circle of Longitude paſſing through the Stars true Place, and equal to the Diameter of the little Circle deſcribed by a Star (as was before ſuppoſed) in the Pole of the Ecliptick; and whoſe Conjugate Axis is to its Tranſverſe Axis, as the *Sine* of the Stars Latitude to the *Radius*. And allowing that a Star by its apparent Motion does exactly deſcribe ſuch an Ellipſe, it will be found, that if A be the Angle of Poſition (or the Angle at the Star made by two great Circles drawn from it, thro' the Poles of the Ecliptick and Equator) and B be another Angle, whoſe Tangent is to the Tangent of A as Radius to the Sine of the Latitude of the Star; then B will be equal to the Difference of Longitude between the Sun and the Star, when the true and apparent Declination of the Star are the ſame. And if the Sun's Longitude in the Ecliptick be reckoned from that Point, wherein it is when this happens; then the Difference between the true and apparent Declination of the Star (on Account of the Cauſe I am now conſidering) will be always, as the Sine of the Sun's Longitude from thence. It will likewiſe be found, that the greateſt Difference of Declination that can be between the true and apparent Place of the Star, will be to the Semi-Tranſverſe Axis of the Ellipſe (or to the Semi-diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick) as the Sine of A to the Sine of B.

If the Star hath North Latitude, the Time, when its true and apparent Declination are the ſame, is before

fore the Sun comes in Conjunction with or Oppoſition to it, if its Longitude be in the firſt or laſt Quadrant (*viz.* in the aſcending Semi-circle) of the Ecliptick; and after them, if in the deſcending Semi-circle; and it will appear neareſt to the North Pole of the Equator, at the Time of that *Maximum* (or when the greateſt Difference between the true and apparent Declination happens) which precedes the Sun's Conjunction with the Star.

Theſe Particulars being ſufficient for my preſent Purpoſe, I ſhall not detain you with the Recital of any more, or with any farther Explication of theſe. It may be time enough to enlarge more upon this Head, when I give a Deſcription of the Inſtruments *&c.* if that be judged neceſſary to be done; and when I ſhall find, what I now advance, to be allowed of (as I flatter my ſelf it will) as ſomething more than a bare Hypotheſis. I have purpoſely omitted ſome matters of no great Moment, and conſidered the Earth as moving in a Circle, and not an Ellipſe, to avoid too perplexed a *Calculus*, which after all the Trouble of it would not ſenſibly differ from that which I make uſe of, eſpecially in thoſe Conſequences which I ſhall at preſent draw from the foregoing Hypotheſis.

This being premiſed, I ſhall now proceed to determine from the Obſervations, what the real Proportion is between the Velocity of Light and the Velocity of the Earth's annual Motion in its Orbit; upon Suppoſition that the *Phænomena* before mentioned do depend upon the Cauſes I have here aſſigned. But I muſt firſt let you know, that in all the Obſervations hereafter mentioned, I have made an Allowance for the Change of the Star's Declination on Account of the Preceſſion of

the Equinox, upon Suppoſition that the Alteration from this Cauſe is proportional to the Time, and regular through all the Parts of the Year. I have deduced the real annual Alteration of Decſination of each Star from the Obſervations themſelves; and I the rather chooſe to depend upon them in this Article, becauſe all which I have yet made, concur to prove, that the Stars near the Equinoctial Colure, change their Declination at this time $1'' \frac{1}{2}$ or $2''$ in a Year more than they would do if the Preceſſion was only $50''$, as is now generally ſuppoſed. I have likewiſe met with ſome ſmall Varieties in the Declination of other Stars in different Years, which do not ſeem to proceed from the ſame Cauſe, particularly in thoſe that are near the ſolſtitial Colure, which on the contrary have altered their Declination leſs than they ought, if the Preceſſion was $50''$. But whether theſe ſmall Alterations proceed from a regular Cauſe, or are occaſioned by any Change in the Materials *&c.* of my Inſtrument, I am not yet able fully to determine. However, I thought it might not be amiſs juſt to mention to you how I have endeavoured to allow for them, though the Reſult would have been nearly the ſame, if I had not conſidered them at all. What that is, I will ſhew, firſt from the Obſervations of γ *Draconis*, which was found to be $39''$ more Southerly in the Beginning of *March*, than in *September*.

From what hath been premiſed, it will appear that the greateſt Alteration of the apparent Declination of γ *Draconis*, on Account of the ſucceſſive Propagation of Light, would be to the Diameter of the little Circle which a Star (as was before remarked) would ſeem to deſcribe about the Pole of the Ecliptick, as $39''$ to $40'',4$. The half of this is the Angle A C B (as repreſented

ſented in the *Fig.*)This therefore being 20″, 2, A C will be to A B, that is, the Velocity of Light to the Velocity of the Eye (which in this Caſe may be ſuppoſed the ſame as the Velocity of the Earth's annual Motion in its Orbit) as 10210 to One, from whence it would follow, that Light moves, or is propagated as far as from the Sun to the Earth in 8′ 12″.

It is well known, that Mr. *Romer*, who firſt attempted to account for an apparent Inequality in the Times of the Eclipſes of *Jupiter*'s Satellites, by the Hypotheſis of the progreſſive Motion of Light, ſuppoſed that it ſpent about 11′ Minutes of Time in its Paſſage from the Sun to us: but it hath ſince been concluded by others from the like Eclipſes, that it is propagated as far in about 7 Minutes. The Velocity of Light therefore deduced from the foregoing Hypotheſis, is as it were a *Mean* betwixt what had at different times been determined from the Eclipſes of *Jupiter*'s Satellites.

Theſe different Methods of finding the Velocity of Light thus agreeing in the Reſult, we may reaſonably conclude, not only that theſe *Phænomena* are owing to the Cauſes to which they have been aſcribed; but alſo, that Light is propagated (in the ſame *Medium*) with the ſame Velocity after it hath been reflected as before: for this will be the Conſequence, if we allow that the Light of the Sun is propagated with the ſame Velocity, before it is reflected, as the Light of the *fixt Stars*. And I imagine this will ſcarce be queſtioned, if it can be made appear that the Velocity of the Light of all the *fixt Stars* is equal, and that their Light moves or is propagated through equal Spaces in equal Times, at all Diſtances from them: both which points (as I apprehend) are ſufficiently proved from the apparent Alte-

ration

ration of the Declination of Stars of different Luſtre; for that is not ſenſibly different in ſuch Stars as ſeem near together, though they appear of very different Magnitudes. And whatever their Situations are (if I proceed according to the foregoing Hypotheſis) I find the ſame Velocity of Light from my Obſervations of ſmall Stars of the fifth or ſixth, as from thoſe of the ſecond and third Magnitude, which in all Probability are placed at very different Diſtances from us. The ſmall Star, for Example, before ſpoken of, that is almoſt oppoſite to γ *Draconis* (being the 35th *Camelopard. Hevelii* in Mr. *Flamſteed*'s Catalogue) was 19″ more Northerly about the Beginning of *March* than in *September*. Whence I conclude, according to my Hypotheſis, that the Diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick would be 40″, 2.

The laſt Star of the great Bear's-tail of the 2d Magnitude (marked η by *Bayer*) was 36″ more Southerly about the Middle of *January* than in *July*. Hence the *Maximum*, or greateſt Alteration of Declination of a Star in the Pole of the Ecliptick would be 40″, 4, exactly the ſame as was before found from the Obſervations of γ *Draconis*.

The Star of the 5th magnitude in the Head of *Perſeus* marked τ by *Bayer*, was 25″ more Northerly about the End of *December* than on the 29th of *July* following. Hence the *Maximum* would be 41″. This Star is not bright enough to be ſeen as it paſſes over my Zenith about the End of *June*, when it ſhould be according to the Hypotheſis fartheſt South. But becauſe I can more certainly depend upon the greateſt Alteration of Declination of thoſe Stars, which I have frequently obſerved about the Times when they become ſtatio-

nary, with reſpect to the Motion I am now conſidering; I will ſet down a few more Inſtances of ſuch, from which you may be able to judge how near it may be poſſible from theſe Obſervations, to determine with what Velocity Light is propagated.

α *Perſei Bayero* was 23″ more Northerly at the beginning of *January* than in *July*. Hence the *Maximum* would be 40″, 2. α *Caſſiopeæ* was 34″ more Northerly about the End of *December* than in *June*. Hence the *Maximum* would be 40″, 8. β *Draconis* was 39″ more Northerly in the beginning of *September* than in *March*; hence the *Maximum* would be 40″, 2. *Capella* was about 16″ more Southerly in *Auguſt* than in *February*; hence the *Maximum* would be about 40″. But this Star being farther from my Zenith than thoſe I have before made uſe of, I cannot ſo well depend upon my Obſervations of it, as of the others; becauſe I meet with ſome ſmall Alterations of its Declination that do not ſeem to proceed from the Cauſe I am now conſidering.

I have compared the Obſervations of ſeveral other Stars, and they all conſpire to prove that the *Maximum* is about 40″ or 41″. I will therefore ſuppoſe that it is 40″$\frac{1}{2}$ or (which amounts to the ſame) that Light moves, or is propagated as far as from the Sun to us in 8′ 13″. The near Agreement which I met with among my Obſervations induces me to think, that the *Maximum* (as I have here fixed it) cannot differ ſo much as a Second from the Truth, and therefore it is probable that the Time which Light ſpends in paſſing from the Sun to us, may be determined by theſe Obſervations within 5″ or 10″; which is ſuch a degree of exactneſs as we can never hope to attain from the Eclipſes of *Jupiter*'s Satellites.

Having

Having thus found the *Maximum*, or what the greateſt Alteration of Declination would be in a Star placed in the Pole of the Ecliptick, I will now deduce from it (according to the foregoing Hypotheſis) the Alteration of Declination in one or two Stars, at ſuch times as they were actually obſerved, in order to ſee how the Hypotheſis will correſpond with the *Phænomena* through all the Parts of the Year.

It would be too tedious to ſet down the whole Series of my Obſervations; I will therefore make Choice only of ſuch as are moſt proper for my preſent Purpoſe, and will begin with thoſe of *γ Draconis*.

This Star appeared fartheſt North about *September* 7th, 1727, as it ought to have done according to my Hypotheſis. The following Table ſhews how much more Southerly the Star was found to be by Obſervation in ſeveral Parts of the Year, and likewiſe how much more Southerly it ought to be according to the Hypotheſis.

1727.	D.	The Difference of Declination by Obſervation.	The Difference of Declination by the Hypotheſis.	1728.	D.	The Difference of Declination by Obſervation.	The Difference of Declination by the Hypotheſis.
		"	"			"	"
October 20th --		4 ½	4 ½	*March* -	24	37	38
November -	17	11 ½	12	*April* - -	6	36	36 ½
December -	6	17 ½	18 ½	*May* - -	6	28 ½	29 ½
- - -	28	25	26	*June* - -	5	18 ½	20
1728				- - -	15	17 ½	17
January -	24	34	34	*July* - -	3	11 ½	11 ½
February -	10	38	37	*Auguſt* -	2	4	4
March - -	7	39	39	*September* -	6	0	0

Hence

Hence it appears, that the Hypotheſis correſponds with the Obſervations of this Star through all Parts of the Year; for the ſmall Differences between them ſeem to ariſe from the Uncertainty of the Obſervations, which is occaſioned (as I imagine) chiefly by the tremulous or undulating Motion of the Air, and of the Vapours in it; which cauſes the Stars ſometimes to dance to and fro, ſo much that it is difficult to judge when they are exactly on the Middle of the Wire that is fixed in the common Focus of the Glaſſes of the Teleſcope.

I muſt confeſs to you, that the Agreement of the Obſervations with each other, as well as with the Hypotheſis, is much greater than I expected to find, before I had compared them; and it may poſſibly be thought to be too great, by thoſe who have been uſed to Aſtronomical Obſervations, and know how difficult it is to make ſuch as are in all reſpects exact. But if it would be any Satisfaction to ſuch Perſons (till I have an Opportunity of deſcribing my Inſtrument and the manner of uſing it) I could aſſure them, that in above 70 Obſervations which I made of this Star in a Year, there is but one (and that is noted as very dubious on account of Clouds) which differs from the foregoing Hypotheſis more than 2″, and this does not differ 3″.

This therefore being the Fact, I cannot but think it very probable, that the *Phænomena* proceed from the Cauſe I have aſſigned, ſince the foregoing Obſervations make it ſufficiently evident, that the Effect of the real Cauſe, whatever it is, varies in this Star, in the ſame Proportion that it ought according to the Hypotheſis.

But leaſt γ *Draconis* may be thought not ſo proper to ſhew the Proportion, in which the apparent Altera-

tion of Declination is increaſed or diminiſhed, as thoſe Stars which lie near the Equinoctial Colure: I will give you alſo the Compariſon between the Hypotheſis and the Obſervations of *n Urſæ Majoris*, that which was fartheſt South about the 17th Day of *January* 1728, agreeable to the Hypotheſis. The following Table ſhews how much more Northerly it was found by Obſervation in ſeveral Parts of the Year, and alſo what the Difference ſhould have been according to the Hypotheſis.

1727.	d.	The Difference of Declination byObſervation.	The Difference of Declination by the Hypotheſis.	1728.	d.	The Difference of Declination byObſervation.	The Difference of Declination by the Hypotheſis.
		"	"			"	"
September -	14	29 1/2	28 1/2	*April* - -	16	18 1/2	18
- - -	24	24 1/2	25 1/2	*May* - -	5	24 1/2	23 1/2
October - -	16	19 1/2	19 1/2	*June* - -	5	32	31 1/2
November -	11	11 1/2	10 1/2	- - -	25	35	34
December -	14	4	3	*July* - -	17	36	36
1728							
February -	17	2	3	*Auguſt* -	2	35	35 1/2
March - -	21	11 1/2	10 1/2	*September* -	20	26 1/2	26 1/2

I find upon Examination, that the Hypotheſis agrees altogether as exactly with the Obſervations of this Star, as the former; for in about 50 that were made of it in a Year, I do not meet with a Difference of ſo much as 2″, except in one, which is mark'd

mark'd as doubtful on Account of the Undulation of the Air, *&c.* And this does not differ 3″ from the Hypotheſis.

The Agreement between the Hypotheſis and the Obſervations of this Star is the more to be reguarded, ſince it proves that the Alteration of Declination, on account of the Proceſſion of the Equinox, is (as I before ſuppoſed) regular thro' all Parts of the Year; ſo far at leaſt, as not to occaſion a Difference great enough to be diſcovered with this Inſtrument. It likewiſe proves the other part of my former Suppoſition, *viz.* that the annual Alteration of Declination in Stars near the Equinoctial Colure, is at this Time greater than a Preceſſion of 50″ would occaſion: for this Star was 20″ more Southerly in *September* 1728, than in *September* 1727, that is, about 2″ more than it would have been, if the Preceſſion was but 50″. But I may hereafter, perhaps, be better able to determine this Point, from my Obſervations of thoſe Stars that lie near the Equinoctial Colure, at about the ſame Diſtance from the North Pole of the Equator, and nearly oppoſite in right Aſcenſion.

I think it needleſs to give you the Compariſon between the Hypotheſis and the Obſervations of any more Stars; ſince the Agreement in the foregoing is a kind of Demonſtration (whether it be allowed that I have diſcovered the real Cauſe of the *Phænomena* or not;) that the Hypotheſis gives at leaſt the true Law of the Variation of Declination in different Stars, with Reſpect to their different Situations and Aſpects with the Sun. And if this is the Caſe, it muſt be granted, that the Parallax of the fixt Stars is much ſmaller, than hath been hitherto ſuppoſed by thoſe,

who have pretended to deduce it from their Obſervations. I believe, that I may venture to ſay, that in either of the two Stars laſt mentioned, it does not amount to 2″. I am of Opinion, that if it were 1″, I ſhould have perceived it, in the great number of Obſervations that I made eſpecially of γ *Draconis*; which agreeing with the Hypotheſis (without allowing any thing for Parallax) nearly as well when the Sun was in Conjunction with, as in Oppoſition to, this Star, it ſeems very probable that the Parallax of it is not ſo great as one ſingle Second; and conſequently that it is above 400000 times farther from us than the Sun.

There appearing therefore after all, no ſenſible Parallax in the fixt Stars, the *Anti-Copernicans* have ſtill room on that Account, to object againſt the Motion of the Earth; and they may have (if they pleaſe) a much greater Objection againſt the Hypotheſis, by which I have endeavoured to ſolve the fore-mentioned *Phænomena*; by denying the progreſſive Motion of Light, as well as that of the Earth.

But as I do not apprehend, that either of theſe Poſtulates will be denied me by the Generality of the Aſtronomers and Philoſophers of the preſent Age; ſo I ſhall not doubt of obtaining their Aſſent to the Conſequences, which I have deduced from them; if they are ſuch as have the Approbation of ſo great a Judge of them as yourſelf. I am,

Sir, Your moſt Obedient

Humble Servant

J. BRADLEY.

POSTSCRIPT.

AS to the Obſervations of Dr. *Hook*, I muſt own to you, that before Mr. *Molyneux*'s Inſtrument was erected, I had no ſmall Opinion of their Correctneſs; the Length of his Teleſcope and the Care he pretends to have taken in making them exact, having been ſtrong Inducements with me to think them ſo. And ſince I have been convinced both from Mr. *Molyneux*'s Obſervations and my own, that the Doctor's are really very far from being either exact or agreeable to the *Phænomena*; I am greatly at a Loſs how to account for it. I cannot well conceive that an Inſtrument of the Length of 36 Feet, conſtructed in the Manner he deſcribes his, could have been liable to an Error of near 30″ (which was doubtleſs the Caſe) if rectified with ſo much Care as he repreſents.

The Obſervations of Mr. *Flamſteed* of the different Diſtances of the Pole Star from the Pole at different Times of the Year, which were through Miſtake looked upon by ſome as a Proof of the annual *Parallax* of it, ſeem to have been made with much greater Care than thoſe of Dr. *Hook*. For though they do not all exactly correſpond with each other, yet from the whole Mr. *Flamſteed* concluded that the Star was 35″ 40″ or 45″ nearer the Pole in *December* than in *May* or *July*: and according to my Hypotheſis it ought to appear 40″ nearer in *December* than in *June*. The Agreement therefore of the Obſervations with the Hypotheſis is greater than could reaſonably be expected, conſidering the *Radius* of the Inſtrument, and the Manner in which it was conſtructed.

ERRATA.

N°. 399. Tab. content. pag. 2. lin. 14. leg. *Curienſis.* N°. id. pag. 294. lin. 19. leg. *latifolia.* ibid. l. 11. leg. *pyri-inciſis.* pag. 315. l. 23. leg. *Curienſis.* pag. 317. l 11. leg. *cuticula.* ibid. l. 17. leg. *Heiſteri.* p. 322. l. 2. leg. *differentes.* N°. 400. p. 351. l. 7. leg. *Caſes.* N°. 403. p. 471. l. 26. leg. *ritè.* ibid. pag. 489. l. 15. leg. *hexaedra.* p. 490. l. 27. leg. *per.* N°. 404. p. 535. l. 14. leg. *thirty-three.*

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FINIS.